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ASPIRING TO A BETTER FUTURE: CAN A SIMPLE PSYCHOLOGICAL INTERVENTION REDUCE POVERTY?

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ABSTRACT

How do aspirations influence investment decisions for people living in poverty? Does this change as peoples economic conditions improve? To answer these questions, we design a workshop teaching techniques to raise aspirations and plan to achieve them. We cross-randomise this with large unconditional cash transfers in a 415-village, 8,300-person, 1.5-year experiment in Kenya. The workshop substantially raises aspirations, investment, and living standards. But the workshop +cash produces similar effects to cash alone, potentially because cash raises aspirations. Thus, helping people living in poverty set higher aspirations can raise investment and living standards, but improving economic conditions can activate the same process.

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A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/996
Videos and workshop materials used in the intervention is available at
https://mbrg.bsg.ox.ac.uk/aspirations-and-goal-setting-video-intervention

1 Introduction

Despite progress in poverty reduction globally, some people and communities struggle to accumulate assets and raise their earnings. While a lack of capital and access to education and markets matter, new evidence suggests psychological factors or "internal constraints" may inhibit people living in poverty from taking up work or investment opportunities, despite potentially high returns.[1] One possible internal constraint is that disadvantaged people or communities may lack opportunities to learn "the capacity to aspire" (Appadurai, 2013): to set higher long-term aspirations to improve their socio-economic position and plan concrete steps to achieve them. They may not encounter institutions or traditions encouraging them to strive and plan for a better future or be less likely to encounter role models who have succeeded. They may set lower aspirations than they are able to attain, limit economic investments and accumulate less wealth than if they had higher aspirations.[2]

This paper provides causal evidence that boosting aspirations among people living in poverty can increase investment and wealth. Isolating the causal effect of aspirations on investment is challenging: few interventions or shocks alter aspirations without also affecting the information or opportunities available. We induce variation in aspirations through a unique 80-minute aspirations and planning (Asp&Plan) workshop which teaches participants simple techniques to set higher aspirations and plan to achieve them. Treatment leads to substantial positive effects on aspirations, monetary and time investments, revenue, and living standards after 17 months, when compared to a placebo workshop. However, the relationship between aspirations and investment changes when people's economic conditions improve, which we show by evaluating the workshop in a separate group of villages where poor households have also been offered large unconditional cash transfers.

We establish these findings using a four-step argument. First, to motivate why we target aspirations, we show that *lower aspirations are associated with lower living standards and, conditional on living standards, lower investment in productive activities.* To show this, we census 32,000 households in 415 villages in rural Western Kenya, randomly sample 8,300 households classified as living in poverty on a proxy means test and survey the primary adult woman in each household.[3] We measure respondents' aspirations as the levels of assets, income, and children's education they *would like their household to reach* a decade or more in the future. Aspirations are strongly associated with living standards, more so than other psychological characteristics: beliefs about one's ability to achieve desired outcomes (e.g., self-efficacy or locus of control), time and risk

[1]For example: Alan and Ertac (2018), Alan et al. (2019), Ashraf et al. (2022), Baranov et al. (2020), Barker et al. (2022), Bhat et al. (2022), Blattman et al. (2017), Campos et al. (2017), Ghosal et al. (2020), and McKelway (2021).

[2]This idea appears in anthropology (Appadurai, 2013; Rapport, 2016), sociology (Willis, 1977; Wilson, 1987), and economic theory on the role of aspirations, neighbourhoods and role models in persistent poverty (Benabou, 1996; Dalton et al., 2016; Durlauf, 1996; Genicot and Ray, 2017; Loury, 2009; Lybbert and Wydick, 2018; Streufert, 2000).

[3]Section 2 explains why we targeted women for interventions and hence for our sample.

preferences, and mental health. Aspirations are also more strongly associated with monetary and time investment, conditional on living standards, than these other psychological characteristics. These associations are consistent with poverty decreasing aspirations and low aspirations deterring investment, although they may not capture causal relationships.[4] Respondents' aspirations are typically higher than but highly correlated with their expectations: respondent-specific distributions of these outcomes they *think their household will reach* (Dominitz and Manski, 1997). But both aspirations and expectations are largely uncorrelated with the other psychological characteristics we measure. Hence we view aspirations and expectations are tightly related concepts and we analyse them together, distinct from these other psychological characteristics.

Second, we provide experimental evidence that *a light-touch intervention targeting aspirations can substantially raise investment, earnings and living standards among households living in poverty.* Our Asp&Plan workshop exposes participants to role models and planning techniques. Participants watch a video about the life stories of two local fictional female role models who improve their economic position by setting higher aspirations; defining concrete, immediate steps to achieve them; and planning for obstacles. They then do facilitated exercises to learn to use these techniques.[5] We study the workshop's impacts using the first two arms of a four-armed village-level field experiment. The Asp&Plan workshop is offered to the primary adult woman in each household in arm 1 of our experiment. It has remarkably large effects on five of six prespecified measures of investment and living standards after 17 months relative to a placebo workshop (arm 2). It increases expenditure on productive inputs by 22% of the placebo group mean, labour supply by 5% and revenue by 11%. Households have higher living standards: consumption increases by 4% and non-land asset value by 6%. All these effects are statistically significant. To achieve these changes, households save more and accumulate more small, non-lumpy assets. They invest mainly in non-farm enterprises, supplying more labour, spending more on inventory and enterprise assets, and adopting new business practices, consistent with research finding "slack" in this sector and region (Egger et al., 2022). Effects vary little by baseline wealth or other dimensions in prespecified and machine learning-based heterogeneity analysis, showing the workshop is effective even for poorer households.

Third, we show it is likely *the workshop operates through the mechanism of boosting women's aspirations and expectations for their household's future economic position.* The workshop has large positive treatment effects on prespecified measures of aspirations and expectations 17 months later. We can also rule out a range of alternative mechanisms, although we cannot rule out that the workshop shifts a mechanism that we do not measure. Effects are unlikely to be due to participants acquiring new information because the Asp&Plan video and exercises are compared to a placebo

[4]Results are robust to measuring aspirations as the level respondents would like to reach minus their current level.

[5]Schools in high-income countries teach some related techniques (EEF, 2021a,b; US Department of Education, 2007) but our participants are unlikely to have been exposed to them, as we explain in Section 4.1.

video and exercises containing the same images and descriptions of economic opportunities, but without the inspiring life stories and psychological techniques. The Asp&Plan workshop has few effects on beliefs about returns to economic activities and does not encourage mimicry of activities in the videos. A series of results suggest effects are unlikely to be driven by experimenter demand. It is unlikely the intervention worked through other plausible psychological mechanisms we measure: it has no effects on self-efficacy, locus of control, time and risk preferences, and mental health.

We present a simple model of reference-dependent utility to explain these results. In this model, either aspirations (Dalton et al., 2016; Genicot and Ray, 2017; Lybbert and Wydick, 2018) or expectations (Kőszegi and Rabin, 2006, 2007) proxy for reference points. Exceeding (missing) one's reference point for future consumption increases (decreases) utility. A higher reference point, induced by the aspirations and planning workshop, causes higher current investment to finance higher future consumption and meet the reference point, matching the treatment effects we observe.

Fourth, we examine the workshop's effects on aspirations and investment among participants facing different economic conditions. Some work on the persistence of poverty hypothesises that people who have lacked opportunities to learn to form high aspirations may struggle to adapt their aspirations when material conditions improve (Durlauf, 1996; Genicot and Ray, 2017; Lybbert and Wydick, 2018). If aspirations are akin to some other traits or preferences – shaped by early life experiences or long-run factors like culture or social hierarchies and difficult to shift – then interventions which offer new resources or skills may not be sufficient to raise low aspirations, limiting their positive impact on living standards. However, others model aspirations as being more malleable – shaped by, as well as shaping, economic conditions (Dalton et al., 2016).

To provide empirical evidence on this debate, we compare participants in villages randomly offered large, lump-sum cash transfers of 2,237 USD PPP and Asp&Plan workshops (arm 3 of the experiment, the "combined intervention") to those in villages offered cash transfers and placebo workshops (arm 4). Transfers are provided by the NGO GiveDirectly, are roughly 60% of mean annual household consumption, and offer a major opportunity for households to make investments and improve their economic position. Given the workshop's positive effects on its own, we test if it can encourage people to invest more of their new resources and further improve their economic outcomes.

We find that *improving economic conditions through cash transfers substantially increases aspirations, crowding out the investment-promoting effect of the workshop.* Both the cash transfer and the combined intervention have nearly equal, positive, substantial effects on aspirations and expectations, labour supply, spending on productive inputs, revenue, consumption and assets.[6] Both in-

[6]The cash transfers' effects on economic outcomes are similar to those in another study of the same programme (Egger et al., 2022). The positive effects on aspirations do not just occur because the cash transfer alters recipients' beliefs about their current position: treatment effects are almost as large on the gap between aspirations and perceived current position.

terventions also have near-zero effects on most other psychological mechanisms. In our model, these results are consistent with cash transfers raising reference points for future consumption – proxied by aspirations – and hence shifting current expenditure toward investment to finance higher future consumption. This interpretation is consistent with an additional empirical result: both cash alone and the combined intervention increase the share of expenditure allocated to investment more than standard lifecycle consumption models would predict. This model suggests that, in the combined arm, the cash transfer's positive effects on aspirations and hence on the propensity to invest crowd out the investment-promoting effect that the workshop has by itself. Importantly, the heterogeneous treatment effects of the workshop would not have predicted this pattern. The workshop is equally effective for households that are poorer versus richer at baseline, relative to the placebo. This shows that the workshop interacts differently with the rapid, unanticipated wealth shock delivered by cash transfers versus existing wealth, highlighting the importance of our cross-cut experimental design.

These results raise the possibility that cash or other resource transfers can shift economic outcomes through both conventional wealth effects and behavioural effects. Across all four arms, we find aspirations are malleable, increased both by the exposure to role models and long term planning techniques in the workshop, and by a change in people's economic conditions.[7] Given these results, both types of interventions might help to overcome the aspirational poverty traps modeled by Dalton et al. (2016) and Genicot and Ray (2017), in which poverty lowers aspirations, which in turn lowers investment and entrenches poverty.

We make two contributions. First, to the literature studying whether low aspirations reduce investment, particularly for people living in poverty. Theoretical work argues that low aspirations may contribute to the persistence of poverty and its concentration in particular groups or communities (Benabou, 1996; Dalton et al., 2016; Durlauf, 1996; Genicot and Ray, 2017; Loury, 2009; Lybbert and Wydick, 2018). Other empirical work studies the effect of changes in aspirations on investment.

Within this literature, *we provide a robust experimental design which more cleanly identifies the effect of a change in the mechanism of aspirations on investment and economic outcomes than previous work.* The design has four key features: we identify the effect of variation in people's aspirations over and above the effect of new information; we use village-level randomisation to reduce risks of spillovers; we show effects do not occur through a range of other plausible mechanisms; and we measure effects on aspirations. We provide strong evidence that, for people living in poverty, an increase in aspirations leads to substantial improvements in living standards.

Most work studying aspirations in developed and developing countries focuses on exposure to successful, relatable role models, showing these can improve educational investment and outcomes.[8]

[7] We do not argue that these patterns will generalise to all cash transfer or anti-poverty programmes and further work should study which programme characteristics lead to these psychological benefits.

[8] For example, on exposure to female political leaders or TV characters in developing countries, see Beaman et al.

However, role model exposure can correlate with other factors which drive outcomes. For example, Chattopadhyay and Duflo (2004) show that female leaders can change both public good provision and aspirations. And teachers may affect aspirations through instruction as well as role model effects (Kearney and Levine, 2020). RCTs partly address these issues: for example, Bernard et al. (2018) and Riley (2019) show role model videos can raise education investment, holding constant the policy environment. But this whole literature cannot test if role models simply provide new information or there is another psychological channel at play. Uniquely, we compare our workshop to a specially designed placebo containing the same factual information; rule out that effects occur through changes in beliefs about returns or mimicry; and collect rich data on multiple psychological mechanisms. Furthermore, both RCTs and teachers are randomised at the individual level, so intervention content might spill over to the control group and or there may be behavioural responses from participants knowing others' treatment status (List, 2011). Indeed, Bernard et al. (2018) find evidence of within-village spillovers. We use village-level randomisation to reduce risks of spillovers. And we measure effects on a broad range of investments and on living standards.

Another set of papers study multiweek training workshops that include content encouraging higher aspirations as well as greater self-efficacy, ambitious mindsets and delayed gratification (Cecchi et al., 2022; McKenzie et al., 2022; Rojas Valdes et al., 2022). While providing important evidence evaluating such workshops, these studies do not identify the particular mechanism on which we focus. The trainings shift multiple psychological mechanisms such as locus of control (McKenzie et al., 2022), "pathways" (akin to self-efficacy) (Cecchi et al., 2022) or religiosity (Rojas Valdes et al., 2022); lead to increases in aspirations only after one to three months that fade out; and are not compared to information-equivalent placebos. They have few effects on savings or investment in specific businesses. In contrast, our workshop provides a high dose of content targeting aspirations, persistently increases aspirations, has near-zero effects on other psychological mechanisms, and has large effects on investment. We also provide the first evidence that aspirations interventions can lead to large effects on living standards.

Our second contribution is to highlight the importance of studying how psychological mechanisms are shaped by changes in people's economic conditions. *We believe ours is the first test of the idea that changes in economic conditions, such as wealth shocks from cash transfers, might cause a different aspirations-investment relationship.*[9] Other work uses similar cross-cut designs to study how poor mental health affects invest responses to cash transfers or livelihoods interventions (Blattman et al., 2017; Angelucci and Bennett, 2021). They study a subpopulation facing severe

(2012), La Ferrara et al. (2012), Jensen and Oster (2009) and Macours and Vakis (2014). On teachers and mentors in developed countries, examples include Carrell et al. (2010), Fairlie et al. (2014) and Kofoed and McGovney (2019).

[9] *Conditional* cash transfers increase aspirations but effects may be due to conditionality-induced exposure to teachers and healthcare workers rather than cash (Chiapa et al., 2012; Fruttero et al., 2021; Macours and Vakis, 2014). Our focus on *unconditional* cash transfers avoids this ambiguity.

psychological challenges which means they benefit little from the economic interventions offered in these trials without intensive mental health treatment, but combining treatment and economic interventions yields some complementarities. In contrast, we use a cross-cut design to show that cash transfers alone can raise aspirations in the general population, alleviating a different psychological constraint to investment. We also highlight how findings from cross-cut designs can differ from those based on heterogeneity analysis in two-arm designs.

Results from this part of the experiment may also reconcile our findings of strong effects from the Asp&Plan workshop with studies which find limited effects of bundled trainings for microfinance clients or agricultural co-operative members on aspirations, savings and business outcomes (Cecchi et al., 2022; McKenzie et al., 2022; Rojas Valdes et al., 2022). Possibly, microfinance or agricultural support have already improved participants' aspirations, just as our cash transfer does, limiting the effectiveness of additional trainings. This highlights the importance of studying general populations as well as populations who have selected into specific programmes (Al-Ubaydli and List, 2015).

Our work complements but differs from work studying intensive interventions to change thinking patterns or non-cognitive skills through repeated exposure and practice, in multi-week training with skilled facilitators.[10] We target a different psychological driver of behaviour which can be boosted through simple techniques by laypeople in one session, potentially because low aspirations result simply from a lack of exposure to role models and practices of long-term planning, rather than thinking patterns, skills or underlying preferences. Our findings have important policy implications. Targeting this particular psychological constraint is highly cost-effective: the intervention pays for itself in benefits to consumption expenditure after 17 months and has a highly favourable benefit-cost ratio relative to other development interventions.

Sections 2 and 3 describe the context, sample and non-experimental relationships between psychological and economic measures. Section 4 describes our experimental design and interventions. Section 5 reports the interventions' economic effects. Section 6 examines potential mechanisms behind these effects. Section 7 presents benefit-cost calculations and discusses policy implications.

2 Context and Sample

We study households living in poverty in all 415 villages in northern Homa Bay and southern Siaya, rural counties either side of Lake Victoria in Western Kenya. The area is one of the poorest in the country (Egger et al., 2022). It is adjacent to areas where previous studies with the NGO GiveDirectly have been conducted (Egger et al., 2022; Haushofer and Shapiro, 2016). The

[10]For example, papers study interventions to build patience and self-regulation (Alan and Ertac, 2018; Alan et al., 2019; Blattman et al., 2017); slow down violent reactions (Heller et al., 2017); boost self-image or self-efficacy (Ghosal et al., 2020; John and Orkin, 2022; McKelway, 2021); build ability to visualise alternative future scenarios (Ashraf et al., 2022); or encourage innovative mindsets (Campos et al., 2017).

population is mainly Luo, Kenya's second largest ethnic group. The area is fairly densely populated, with 395 people per km^2 compared to 91 for the country as a whole. Villages contain 96 households on average. Villages have fairly good market access: most are within a few hours' drive of Kisumu, Kenya's third largest city, and closer to smaller towns. Roughly half of the villages contain a primary school, one third contain a market, and one sixth contain a healthcare clinic (Table B.2).

2.1 Target Population

Our population of interest is households living in poverty. To identify them, we conduct a census of 41,322 households in the 415 villages and collect data for a simple proxy means test used by GiveDirectly to assess if households are eligible for cash transfers. The test uses easy-to-collect measures of housing quality and asset ownership, such as the size of the house and floor material, which GiveDirectly has found to be strong predictors of living below poverty lines defined in terms of consumption expenditure. 43% of censused households are classified as living in poverty on this test. Of the households classified as poor on the means test, 89% have per capita consumption in our survey data (described below) below the World Bank's 2018 poverty line for Kenya.

Among households means-tested as poor, we sample only households with an adult woman and target them for our interventions. We focus on adult women because in this area, as in many parts of rural Africa, female-headed households are common and often poor: they are 32% of all households and 41% of means-tested-poor households.[11] Targeting our interventions at female heads of household or married women in couple-headed households enables us to design workshops targeted at one gender (by producing videos featuring female role models and hiring female staff to facilitate workshops) and include a high proportion of households living in poverty in these villages. We draw a random sample of roughly 20 households per village that contain an adult woman, are non-polygamous, and are means-tested as poor.[12] This results in a baseline sample of 8,309 adult women. See Appendix B for details on the census, sampling, and eligibility rules.

2.2 Sources of Data

We ran baseline surveys an average of 5 months before treatment and endline surveys an average of 17 months after treatment, finishing before the COVID-19 pandemic. We completed all baseline surveys for all villages in an area before any interventions began or were announced. We surveyed the "primary adult woman" in each household: the female spouse in a couple-headed household or the widowed or single household head. We surveyed 87.1% of the baseline sample at endline. Attrition is balanced on treatment and treatment times baseline household characteristics (Appendix B).

[11] This demographic structure occurs because there were much higher death rates among men during earlier waves of the HIV/AIDS pandemic (Sifuna et al., 2018). Single or widowed Luo women often do not have land rights and are thus poorer (Potash, 1978).

[12] We drop 4.5% of poor households without an adult woman and 11% of poor households which are polygamous.

The baseline and endline surveys covered three prespecified household-level investment measures – labour supply, expenditure on productive inputs and hired labour, and education expenditure – and three prespecified economic outcomes – revenue, consumption expenditure, and asset value.[13] We chose these three investment types because they are covered in the aspirations and planning workshop and are available to almost all households. Most measures are adapted from the Living Standards Measurement Surveys and Indonesian Family Life Survey. We also measure multiple psychological characteristics, discussed in Section 3.1. See Appendix G for details on all measures.

2.3 Sample Descriptives

We report summary statistics for the endline placebo sample in Table 1. We use endline so that we use the same time period and survey instrument as the treatment effects analysis; baseline statistics for the full sample are similar (Table B.2). The average respondent in the placebo group is 41 years old with an interdecile range of 23-65. The average household contains 2.8 adult members and 2.8 children, which includes biological and non-biological children of younger respondents and grandchildren of older respondents. 59% of the female respondents are married and the remainder are single (widowed, divorced or never married). Only 43% of respondents have completed primary education.

Households are living in poverty based on common metrics. Households consume an average of 3,796 USD PPP annually, or 2.5 USD PPP per adult equivalent per day. Households own non-land assets worth 1,529 USD PPP on average, less than half of the value of their annual consumption. The average household owns another 5,638 USD PPP of land and housing assets, although the values of land and housing are difficult to measure accurately as land transactions are rare.

Households engage in multiple economic activities: 95% of households grow crops, most commonly maize; 83% raise livestock, most commonly chickens; 40% do casual or salaried work outside the household; and 44% operate a non-farm enterprise, most commonly retail (50%), manufacturing (23%), services or fishing (11% each). Households have three main avenues for investment: labour supply, spending on inputs for home production, and children's education. The average household supplies 525 days of labour per year or 216 days per adult member per year, with almost all adult members doing some work, and spends 857 USD PPP per year on productive inputs (Table 1).

Households' labour supply and input expenditure generates mean annual revenue of 2,101 USD PPP.[14] Our sample, a general population living in poverty, differs from previous work on aspirations, which has studied groups with a potentially high propensity for investment, like

[13]The pre-analysis plan is posted at https://www.socialscienceregistry.org/trials/996. Appendix F explains our few departures from this plan.

[14]Our consumption measure is higher on average than our revenue measure, a common pattern in agricultural household surveys (e.g. Bandiera et al. 2017; Egger et al. 2022). We include the value of goods produced for home consumption in both the consumption and revenue measures.

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)
	Mean	Std dev.	10th percentile	90th percentile
Demographics				
Respondent Age	40.6	16.5	23.0	65.0
Household Size	5.56	2.53	2.00	9.00
Number of Children	2.77	1.86	0.00	5.00
Married	0.591	0.492		
Primary School Education	0.426	0.495		
Economic Investment				
Education Expenditure	640	940	42	1,703
Enrolment Rate for Ages 6-13	0.978	0.124	1.000	1.000
Enrolment Rate for Ages 14-20	0.788	0.343	0.000	1.000
Labour Supplied (days)	857	1,932	23	1,973
Expenditure on Productive Inputs	525	347	84	960
Economic Outcomes				
Revenue	2,101	3,204	165	4,708
Consumption	3,796	1,959	1,643	6,407
Consumption per Adult Equivalent	928	579	397	1,605
Value of Non-land Assets	1,529	1,506	289	3,219

Notes: This table shows endline summary statistics for the 7243 households of the placebo group. All currency values are measured in 2018 USD PPP. All flow measures except education expenditure are in annual terms. Details on measurement are given in Appendix G. Education expenditure is all fee and non-fee expenditure in the current and preceding school years summed over all household members aged 6-20. It is set to missing for households with no members aged 6-20. Labour supply is days of work on farm and non-farm household enterprises or supplied to the market, for all household members older than 15. Input expenditure includes purchase of inputs and stocks and inventory, rental, maintenance, and expenditure on hired labour, for household activities in crop agriculture or livestock rearing, or for non-farm enterprises owned or operated by household members. Revenue captures the value of production sold or consumed at home from these activities, valued at farm-gate prices. Consumption expenditure captures the value of purchased and home-produced food, nondurable and durable household goods, and social expenditures following Deaton and Zaidi (2002). Consumption per adult equivalent is adjusted for household demographic composition using adult equivalent scales for Kenya following Anzagi and Bernard (1977). Non-land assets are the estimated value, if sold, of durable assets, livestock, and stocks of dried maize, as well as cash savings. We value output and expenditure on inputs following the Living Standards Measurement Surveys (Grosh and Glewwe, 2000) and UN System of National Accounts (FAO, 1996).

microenterprise owners, microfinance clients or farmers producing for market.[15]

The average household spends 640 USD PPP per year on education, 183 USD PPP per child, mostly on school fees. Kenyan public primary and secondary schools nominally eliminated fees in 2003 and 2008 respectively but many still impose costs on parents and private schools charge fees (Glennester et al., 2011). School enrollment is 98% and 79% respectively for primary and secondary school-aged household members, so there is limited scope for our treatments to affect enrollment.

[15]Revenue from one household business in Rojas Valdes et al. (2022) is three times what our households earn in all economic activities. Farmers in Cecchi et al. (2022) own a mean of 9 cattle; our households own less than one.

3 The Psychological Correlates of Investment and Wealth

To motivate the design of the workshop, we explore which psychological factors might constrain labour supply, investment, or wealth for the people we study. We identify and measure seven psychological characteristics which predict investment or wealth in other contexts: aspirations and expectations for one's future economic position, beliefs about one's ability to carry out actions or achieve desired outcomes, beliefs about returns to specific investment activities, time and risk preferences, and depression. We describe their relationships with wealth and investment in our context. Appendix G provides more details on measurement.

3.1 Definitions and Measurement of Mechanisms

Aspirations for One's Future Economic Position: We define aspirations as the set of future outcomes that people prefer and aim for. We measure aspirations as the level of economic outcomes respondents would like their household to attain in three domains: the levels of assets and income the respondent would like their household to reach in ten years, and the level of education they would like one of their children to reach in adulthood. The child is the child aged closest to 14 from the roster of resident household members. We chose these dimensions because they are relevant to almost all respondents and because, in qualitative scoping work, most respondents described a better life in terms of improvements on these dimensions. We use an inverse covariance-weighted average over these three dimensions to capture a more general aspirational mindset.[16] Similar measures of aspirations predict higher savings and education investment (Janzen et al., 2017; Ross et al., 2021; Beaman et al., 2012) and small business investment (Dalton et al., 2018).

Aspirations can refer to more specific long-term goals (Locke and Latham, 2002): for example, one family in the video aspires to earn enough to put a tin roof on their house. We do not use these as our primary measure of aspirations as they are difficult to compare between respondents or over time. However, we measure goals in the workshop and combined groups after the workshop, and find that respondents with a specific goal in a domain also have higher levels on our quantitative metric of aspirations. See Appendix A.4 for details. Other studies of workshops targeting aspirations, locus of control and self-efficacy measure aspirations differently, asking whether it is desirable to have aspirations or goals, content directly covered in the workshops.[17] We prefer our measures as they map more tightly to our direct theoretical interpretation of aspirations as reference points (see Section 6.3.)

Expectations: We measure respondents' expectations about their possible distributions

[16] Averaging multiple measures of related concepts to improve precision is common in aspirations research (Beaman et al., 2012; Bernard and Taffesse, 2014; Janzen et al., 2017) and econometrics in general (Schennach, 2022).

[17] Example items from their studies include "It is better to have aspirations for your family than to accept each day as it comes" (Cecchi et al., 2022; Rojas Valdes et al., 2022) or "What is a dream timeline?" [a tool taught in the workshop] (McKenzie et al., 2022).

of assets and income over the same time period as aspirations and estimate the individual-specific means of these distributions, following Dominitz and Manski (1997) and Delavande et al. (2011). We also measure the level of education respondents expect a child to reach in adulthood (the same child as in the aspirations measure) and combine the three measures into an inverse covariance-weighted average. Such measures are widely found to affect investment (Delavande, 2022).

Self-beliefs: "Self-beliefs" are people's beliefs about their ability to achieve desired outcomes (Locke and Latham, 2002). We measure three different self-beliefs. Generalised self-efficacy is a person's belief about their ability to carry out actions and achieve their goals across domains (Bandura, 1977). Locus of control is a person's beliefs about whether their actions or fate determine outcomes (Rotter, 1966). Growth mindset is a person's belief that their skills can be altered through effort (Dweck, 2012).[18] We measure these beliefs on psychological scales validated in other contexts, which we translated and back-translated into Luo and validate in a separate study, showing that they have appropriate psychometric properties (see Appendix G.3). We combine the three measures in an inverse covariance-weighted average, as they are correlated in our sample and in other work (Bong and Skaalvik, 2003). These measures predict labour supply, job search, saving, and educational effort in existing research (Caliendo et al., 2015; Dweck, 2012; Heckman and Kautz, 2012; John and Orkin, 2022; Lindqvist and Vestman, 2011; McKelway, 2021).

Beliefs about Returns to Investment: We measure respondents' beliefs about the returns to three potential investment activities: using more labour on their farm, using more fertiliser on their farm, and a university degree (vs secondary education) for their child. For example, we elicit beliefs about the levels of maize output if a farmer works 12 hours per week more for a given land size and set of inputs, and calculate the percentage change they expect relative to working their current hours. We picked these activities because most households face choices about whether to invest in them. Beliefs about these individual returns are broadly realistic, in that they are aligned with measures of actual returns from other work in this literature, as we discuss in Appendix G.2.2. Beliefs about returns to investment and/or labour supply predict investment in education, migration and economic activity in other settings (see Delavande (2022) for a review.)

Present Bias and Patience: We measure patience and present bias with money from an incentivised seven-choice multiple price list with choices over tomorrow vs 15 days and 15 vs 29 days (Andersen et al., 2008). Individuals who place higher utility weight on future relative to current consumption have higher savings and educational attainment in many contexts (Falk et al., 2018).

Propensity for Risk-Taking: We create an ordinal measure of risk-taking from an incentivised Eckel and Grossman (2002) measure. Propensity to take risks is correlated with higher levels

[18]These are closely related to measures of self-confidence, self-esteem or identity as a competent person (Rosenberg and Kapland, 1982).

of technology adoption and business ownership, potentially because it lowers the value of certain consumption and leisure today relative to uncertain future outcomes (Falk et al., 2018; Liu, 2013).

Mental Health: We focus on depression, a common mental health condition, measured with the standardised 10-item Centre for Epidemiological Studies-Depression (CESD) scale (Andresen et al., 1994). Symptoms include pessimistic beliefs, impairment in motivation and ability to perform everyday activities, and dejected mood (Beck and Alford, 2009). Treatments which reduce depression improve labour supply and investment, productivity at work, savings and education investment (Angelucci and Bennett, 2021; Baranov et al., 2020; Lund et al., 2022). We reverse-code the scale so higher scores indicate higher mental health.

3.2 Relationships Between Psychological Characteristics and Investment and Wealth

In this section we describe four patterns in the relationships between investment, wealth, and psychological characteristics that motivate our intervention and conceptual framework. First, aspirations are strongly positively associated with wealth. To show this, we use endline placebo group data to regress respondents' wealth on their aspirations, self-beliefs, beliefs about returns, time and risk preferences, mental health, demographic controls, and county fixed effects, with all psychological measures standardised to facilitate comparison of their coefficient magnitudes. This regression shows that a one standard deviation increase in aspirations is associated with 247 USD PPP more non-land assets (Figure 1, top panel). This pattern by itself has many possible interpretations: aspirations might motivate behaviour that generates wealth, wealthier people might anticipate better outcomes, or some other factors might drive both aspirations and wealth.

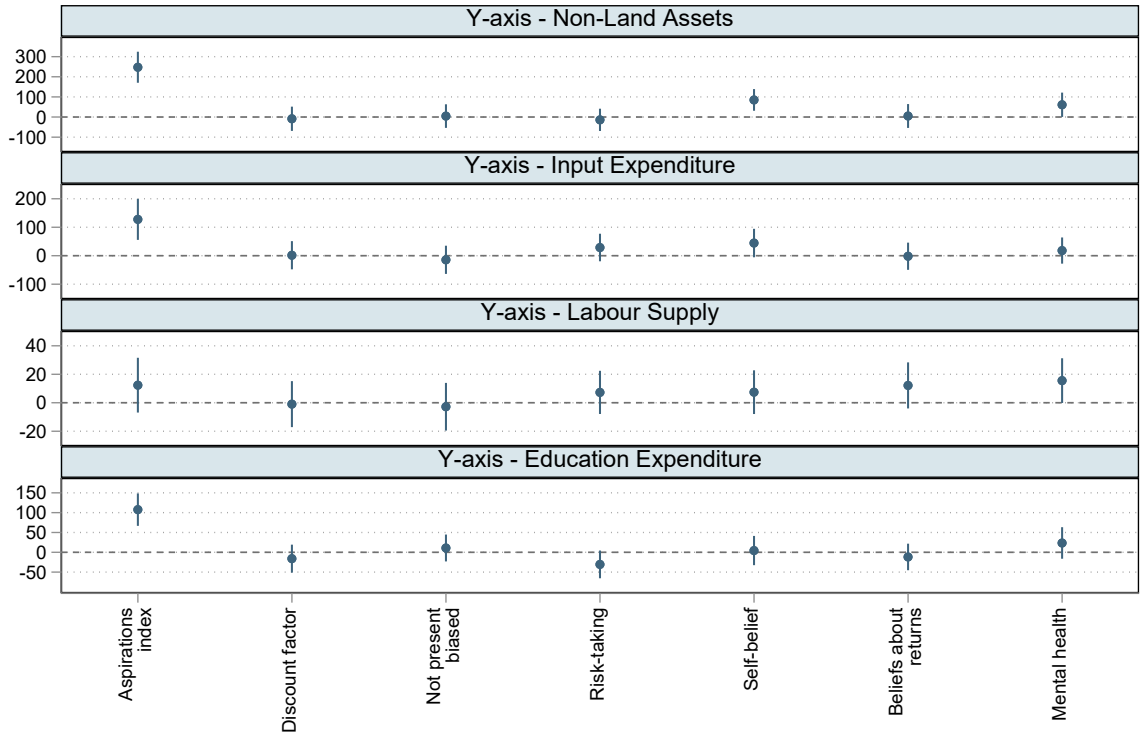
Second, aspirations are strongly positively associated with investment, conditional on wealth. To show this, we regress investment on the variables listed above, also controlling for consumption and non-land asset value, and show results in the bottom three panels of Figure 1. The two additional controls allow us to compare investment by households with different aspirations but the same (proxies for) wealth. A one standard deviation increase in aspirations is associated with 128 USD PPP more input expenditure, 12 more days of household labour supply (not statistically significant), and 108 USD PPP higher education expenditure.[19] Jointly, these first two patterns are consistent with higher aspirations motivating investment and hence raising wealth, although other interpretations are obviously possible.[20]

Third, the same regressions show that other psychological characteristics are at most weakly associated with investment and wealth. Wealthier people have higher self-beliefs and mental

[19]We also find that domain-specific aspirations are related to domain-specific investment. For example, education expenditure is positively associated with education-specific aspirations.

[20]Eble and Escueta (2022), Guyon and Huillery (2021), Janzen et al. (2017), La Ferrara (2019), Ross (2019) and Ross et al. (2021) document positive correlations between aspirations and investment in other settings.

Figure 1: Relationships Between Wealth, Investment, and Psychological Characteristics



Notes: This figure shows coefficients and 95% confidence intervals from regressing different wealth and investment measures on psychological characteristics. The first row show results from regressing a wealth proxy (non-land assets) on respondents' aspirations, self-beliefs, beliefs about returns, time and risk preferences, mental health, age, education, marital status, household size, number of school-aged members, and county fixed effects. The second, third, and fourth rows show results from regressing investment measures (respectively expenditure on productive inputs and hired labour, labour supply, and education expenditure) on the same controls as in the first row, plus two wealth proxies: consumption and non-land asset value. All asset and investment measures are defined in the footnote below Table 1. The aspirations index and psychological measures are defined in Section 3.1. All psychological measures are standardised to simplify comparison of the coefficients. All regressions use the endline placebo group data with the top percentiles of aspirations, investment, assets, and consumption trimmed. Sample size is 1376 to 1747 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

health but these relationships are far smaller than the aspirations-wealth relationship (Figure 1, top panel). None of the three investment measures is strongly associated with any non-aspirations psychological characteristic, conditional on wealth (Figure 1, bottom three panels).

Fourth, our expectations index has almost the same relationship with wealth and investment as our aspirations index (Figure A.1). To show this, we repeat the analysis above replacing the aspirations index with the expectations index. We run aspirations and expectations analyses separately rather than including them in the same regression because the two are highly correlated

(ρ=0.6) and conceptually similar, as we show in the next subsection.

These four patterns motivate a focus on aspirations and expectations as potentially important psychological influences on investment and wealth in this context. They are also consistent with a model we present in Section 6.3, where aspirations or expectations proxy for reference points for future consumption and people with higher reference points invest more to accumulate higher wealth and afford higher consumption in the future. However, these four patterns don't prove a causal effect of aspirations on investment and wealth, for which we turn to the experimental analysis.

The patterns are highly robust. They are similar when we remove all controls, remove all psychological characteristics except aspirations, or add respondent fixed effects using the panel structure (Figure A.2). The investment relationships hold when we replace aspirations (or expectations) with the difference between aspirations (or expectations) and respondents' self-reports about their current assets, income, and education (Figure A.3). This shows that the positive aspirations-investment association does not simply occur because people with higher wealth, or perceived wealth, have higher aspirations. The patterns hold using baseline data on aspirations, although the baseline data does not capture time and risk preferences (Figure A.4).[21] The relationships between aspirations, investment and wealth are similar when we control for cognitive ability using a subsample of data.[22] Standardised ridge regressions, a method to measure predictive power, classify aspirations as the strongest predictor of both wealth and investment.

3.3 Relating Aspirations, Expectations, and Other Psychological Concepts

In our sample, aspirations are closely related but not identical to standard economic measures of expectations, which elicit what respondents believe will occur. Our measures of aspirations capture something close to the upper limits of outcomes respondents believe are possible. The average respondent has asset and income aspirations at respectively the 71st and 66th percentiles of their individual expectations distribution. Only 30% of respondents have aspirations above the maximum of their expectations distribution and only 15% have aspirations below the mean of their expectations distribution. Patterns are similar for the level of children's education: 72% of respondents have equal aspirations and expectations and 21% of respondents have higher aspirations than expectations.

In our sample, respondents report aspiring to modest improvements to their current posi-

[21] We also estimate the aspirations-investment and aspirations-wealth relationships using splines to allow for nonlinear relationships. We cannot reject the null hypothesis of linearity for any relationship. Some papers have hypothesised or documented inverse-U-shaped aspirations-investment relationships, which they interpret as high aspirations motivating higher investment and very high aspirations leading to frustration and lower investment (Genicot and Ray, 2017; Janzen et al., 2017). We find no evidence of an inverse-U-shaped relationship in our data.

[22] We only collect cognitive ability for a subsample to use as a robustness check because it is unlikely to respond to a short workshop with adults (Heckman et al., 2006). We find that cognitive ability is positively associated with wealth and investment, echoing the finding from Laajaj and Macours (2021) in the same context.

tion. Aspirations are higher than respondents' estimates of their current assets and income, but not dramatically, with median ratios of aspirations to current levels of 3 and 4.2 respectively.[23] Respondents' aspirations are lower than their beliefs about the highest level of income and assets someone from their village could reach in ten years: the median ratio of own aspirations to this "village maximum" is 0.5 for assets and 0.4 for income.

Aspirations are not simply close correlates of other mechanisms we measure: they have pairwise correlations ≤ 0.1 with all other psychological concepts besides expectations (Table A.16). Regressing aspirations on all the other psychological concepts explains only 2% of the variation in aspirations and a LASSO estimation approach gives similar conclusions (Table A.17).

4 Experimental Design and Interventions

Descriptive patterns in this sample suggest that low aspirations and expectations for future living standards might constrain investment and wealth accumulation. We hence run an experiment to test if our Asp&Plan workshop also increases investment and wealth. We randomly assigned each of the 415 villages to one of four treatments: a placebo workshop, the aspirations and planning workshop ("Asp&Plan"), an unconditional cash transfer and placebo workshop ("Cash"), or the cash transfer and Asp&Plan workshop ("Combined"). Village-level randomisation was stratified on village size, amenities, and poverty rate: details are in Appendix B.2.

We use this four-armed design to make two comparisons. First, we compare the Asp&Plan workshop to the placebo workshop. This comparison captures the effect of the psychologically active elements of the workshop, holding constant the effects of common elements in both workshops, such as information or interaction with facilitators. Second, we compare the cash and combined groups to capture the effect of the aspirations and planning workshop in the presence of the large wealth shock delivered by the cash transfer.

The workshops are delivered to the "primary woman" in each household, who also completes our surveys. The cash transfers are delivered to the mobile money account of one household member, chosen jointly by the adults in the household; 91% of study households choose the primary woman. All interventions are delivered in the same month to households in each geographic location. The workshops involved two ten-minute videos shown on a tablet, followed by facilitated exercises, lasting a median of 33 minutes, for a total workshop time of roughly 60–90 minutes.[24]

[23] Respondents' summary estimates of their current assets and income are, in turn, similar to measures of their current assets and income we construct from their detailed survey responses. The median ratio of respondents' direct summary reports to the measures we construct is 0.75 for assets and 0.93 for income.

[24] The interquartile range of the duration of exercises is 16–47 minutes. All videos are posted at https://mbrg.bsg.ox.ac.uk/aspirations-and-goal-setting-video-intervention. Appendix C contains a summary of the aspirations and planning workshop videos and the full exercise script.

Workshops are mainly administered to randomly assembled groups of three to four people with those who missed meeting times receiving them individually.

4.1 Aspirations and Planning Workshop

We developed a unique aspirations and planning workshop to achieve three aims: to raise participants' aspirations, while keeping them attainable; to encourage them to form specific long- and short-term plans to achieve these aspirations; and to encourage them to take actions in the present to begin working towards their aspirations. Workshops contain the following four elements.

Aspirational Role Model Videos: The videos narrate the life stories of two fictional women from a similar area to the participants and from poor backgrounds. They succeeded in improving their socio-economic position by forming ambitious but attainable aspirations and working toward them over a number of years despite obstacles. In one story, Judy and her husband Oyoo aspire to own a house with an iron roof and send their children to higher levels of education. In the medium term, they plan to sell vegetables, for Judy to train to be a tailor, and to save more. Judy struggles to learn to sew, opens a sewing business and faces heavy competition, but eventually she establishes the business and achieves her aspirations. The second story, of Josefine, describes her journey from being a day labourer to continuing her education. The characters also "model" the techniques for goal-setting and planning that the participants then learn in the exercises and link their use to achieving one's long-term goals or aspirations.

We use life stories because health research suggests watching a relevant person "modelling" behaviour makes viewers more engaged and changes attitudes more than merely receiving information (La Ferrara, 2016; Murphy et al., 2014). Social learning theory in psychology suggests role models are particularly influential in the formation of aspirations because people form aspirations with reference to the outcomes of other similar individuals (Bandura, 1977). We make narratives inspirational and emotional, aiming to provide a "vicarious experience" (Bandura, 1977): a resonant and influential experience of the life of another similar person. Importantly, the characters are shown making modest progress over many years and facing difficulties and disappointments. This draws on psychology research which finds role models are most inspiring when people believe their success is attainable and are shown the process for achieving success (Lockwood and Kunda, 1997; Marx and Ko, 2012). We target workshops at one gender so characters in the videos seem relevant to participants.

Best Possible Selves: After the videos, participants did a series of exercises. In the first exercise, participants are asked to think about and describe their lives in five years "after everything has gone as well as it possibly could" to inspire setting of aspirations (King, 2001). They are encouraged to think of "the realisation of their dreams" where they have "reached their full potential". Fieldworkers recorded the goals participants described.

Personal Goal-setting: In the second exercise, fieldworkers relay participants' goals back to them. Participants select their most important and achievable goal and report how long it would take to achieve it (Morisano et al., 2010). Appendix A.4 describes the diverse topics of these goals: 78% of respondents report at least one goal related to farming (mostly raising a specific type of livestock) and the shares are 86% for housing, 50% for education, and 37% for non-farm enterprises. We did not promote goals in any specific domain.

Mental Contrasting, Implementation Intentions, and Anticipation of Obstacles: In a third exercise, participants formulate "implementation intentions": a concrete step they could take in the next week towards their goal (Gollwitzer and Sheeran, 2006). They identify potential obstacles to their goal and strategies to overcome these obstacles (Duckworth et al., 2013). They engage in "mental contrasting:" identifying how their lives would be improved and how they would feel if they achieve their aspiration (Oettingen and Gollwitzer, 2010).[25] Participants are given a reminder calendar, which shows the characters and sayings from each video, on which they placed a sticker to remind them of their goals.

The videos were filmed by a production company based on our scripts, with ordinary people who were paid to be amateur actors, near the study location. The stories are compilations of life stories of real people we collected in qualitative work. The videos were extensively pilot-tested in two rounds of focus groups and adapted. The intervention also draws elements from studies in psychology. We reviewed the psychological literature, mainly searching the American Psychological Association's PsycNet database, to identify short exercises that targeted one of the goals of the intervention. Each of the exercises has, on its own, had effects on lab tasks, intentions, healthy behaviours or education effort after one to two months in richer countries (Cross and Sheffield, 2019; Duckworth et al., 2013; Loveday et al., 2018). Some related techniques are taught in schools in high-income countries (EEF, 2021a,b; US Department of Education, 2007) but our participants are unlikely to have been exposed to them. Goal-setting and planning and were included in the Kenyan primary school life skills curriculum in 2017 (KICD, 2017), but our participants left school before the curriculum was introduced and only 43% completed primary school.

4.2 Placebo Intervention

The placebo intervention includes two videos showing and describing all activities in the aspirations and planning video. They describe farming in rural Kenya and education and types of work, mirroring the information available in the stories of Judy and Josefine. We made these videos by

[25] In our pre-analysis plan, we stated the videos have elements to encourage a growth mindset (Dweck, 2012). However, subsequent work suggests growth mindset interventions must be highly specific and focus on specific ideas about neuroplasticity of intelligence (Yeager and Dweck, 2020), which our interventions do not.

including at least one shot of every scene and character from the Asp&Plan video but cutting out the "psychologically active" components – descriptions of characters or development of their stories, shots of people conveying emotion, or music – and replacing dialogue or narrative voiceovers with factual descriptions of the content of the scene. This is followed by exercises of similar length to the psychological techniques. In these, respondents recall and discuss the videos' content, accuracy and quality but do not learn the Best Possible Selves, goal-setting, mental contrasting, implementation intentions or anticipation of obstacles techniques. The placebo design rules out that effects are caused by participants learning about economic activities from the videos. It also rules out effects of being selected for an intervention by people from outside the community or interacting with a facilitator, as this occurs in both interventions. Finally, it rules out the effects of meeting with a group, as both interventions have the same group structure.

4.3 Cash Transfers

The second intervention is a large, lump-sum unconditional cash transfer, delivered by our implementation partner GiveDirectly, of 2,237 USD PPP, equal to 59% of mean annual consumption expenditure and 146% of mean non-land asset value in our sample. The transfer was delivered in three tranches of 203, 1,017 and 1,017 USD PPP one month apart via the mobile money payments system M-Pesa. The total transfer amount delivered is comparable to the amounts given by current government pilot programmes targeted at the ultra-poor operating in some other regions: the Kenyan government's Hunger Safety Net Programme pays out the equivalent of the GiveDirectly transfer in 21 months (Kenya National Social Protection Secretariat, 2022).

After randomisation, GiveDirectly began operations in cash villages with a meeting to explain their programme, including that it targeted poor households and that transfers were from an independent non-governmental organisation. GiveDirectly then visited all eligible households to offer them the transfer, making an extra visit if households were initially not at home. Households who signed up were asked to register for M-Pesa at a network of agents in most small stores.[26] Each household picked one member whose M-Pesa account would receive the transfer: 91% picked the adult woman, who we also target for the workshop.

4.4 Timing of Interventions and Experimental Integrity

We rolled out all interventions within a sublocation, an administrative unit containing on average 10 contiguous villages, in the same month. Once a whole sublocation had been censused, baselined and registered, GiveDirectly sent transfers by mobile money on the 15th day of each month to that sublocation. Workshops were administered in the month around the date when GiveDirectly

[26]If an eligible household did not have a mobile phone to access M-Pesa, GiveDirectly provided one and subtracted its cost from the value of the transfer.

sent the first tranche of the cash transfer to villages receiving cash. All cash-eligible households knew they were receiving cash by the time they attended a workshop.

We report on balance, attrition and compliance in Appendix B. Attrition is balanced across treatment arms, only weakly related to baseline household characteristics, and balanced across treatment times baseline household characteristics (Table B.1). The treatment assignments are balanced on village-, household-, and respondent-level characteristics measured before treatment (Table B.2). Take-up of treatments is balanced within the two pairs of arms used for our two main comparisons: between the 'Asp&Plan' workshop and the placebo workshop arms, and between the cash and combined arms. So differences between these two pairs of interventions do not result from differences in intervention take-up. See Appendix B.2 for further discussion.

4.5 Estimation and Inference

We estimate models of the form

$$Y_{iv} = \text{Cash}_v \cdot \beta_C + \text{Asp\&Plan}_v \cdot \beta_P + \text{Combined}_v \cdot \beta_{CP} + \mathbf{X_{iv}} \cdot \mathbf{\Gamma} + \epsilon_{iv}, \tag{1}$$

where i and v index individuals and villages. Y_{iv} is the post-treatment outcome of interest at endline. Cash_v, $\text{Asp\&Plan}_v$, and Combined_v are indicators for assignment to respectively cash transfers, the aspirations and planning workshop, and the combined intervention. $\mathbf{X_{iv}}$ contains prespecified covariates and sublocation fixed effects.[27] The covariates make little difference to the estimated treatment effects but lower the estimated standard errors. We report heteroskedasticity-robust standard errors clustered by village, the unit of treatment assignment. We control the false discovery rate across multiple tests by reporting sharpened q-values for the effect of each treatment and for tests of equality of each pair of treatment effects (Benjamini et al., 2006). We run all analysis at the household level except some individual-level education analyses in the appendices.

Our estimation and inference methods and outcome measures are prespecified at https://www.socialscienceregistry.org/trials/996. We make a few departures from the pre-analysis plan to improve comparability across economic aggregates and remove components with high measurement error. We list these in Appendix F and highlight two here. First, we adjust for multiple testing across the six economic aggregates and across all the main psychological mechanisms. This is more conservative than the prespecified adjustment, which was only across the prespecified components of each aggregate or index. Second, we summarise results by constructing a non-prespecified inverse covariance-weighted average of the six economic aggregates, following Anderson (2008).

[27] The prespecified covariates are month-of-endline fixed effects (to account for seasonality); the baseline values of Y_{iv}; baseline household size, asset value, a self-beliefs index (made up of locus of control and self-efficacy scales, defined in Appendix G), respondent education, respondent age; and an indicator for the endline being answered by a proxy respondent.

5 Effects on Economic Outcomes

Table 2 shows treatment effects of the three interventions on six prespecified economic aggregates. To help interpret these results, we also discuss treatment effects on some components of these aggregates and show the component results in Appendix A. None of the interventions affect household size or composition, including fertility.

5.1 Economic Effects of the Aspirations and Planning Intervention

The aspirations and planning workshop has substantial effects on households' economic activities – increasing three types of investment – and outcomes of these activities – increasing revenue, asset value, and consumption. The workshop increases households' annual labour supply by 27 days per year (5% of the placebo mean, Table 2, column 2). This is driven by roughly equal increases in labour supply by the respondent and other adult household members, with no effect on children's labour supply. It also increases annual expenditure on intermediate inputs for home production and hired labour by 230 USD PPP (27% of the placebo mean, column 3). Annual household-level education spending increases by 22 USD PPP (3.5% of the placebo mean, column 4), although this is not statistically significant ($p = 0.42$). A prespecified breakdown by age group shows that this is driven by an 11% rise in spending per primary school-aged child ($p = 0.07$, Table A.2). The treatment effect on expenditure for post-primary school-aged children is close to zero, potentially because secondary school costs are larger and lumpier (Lucas and Mbiti, 2012). Treatment effects on both enrolment and attendance are negligible, perhaps in part because these are high at all ages (Table A.1).

The Asp&Plan workshop increases annual revenue by 260 USD PPP: 12% of the placebo mean (Table 2, column 5). The labour supply, input expenditure and revenue effects are concentrated in non-farm enterprises: households supply 9 days more labour per year to these enterprises, spend 174 USD PPP more on inputs, earn 284 USD PPP more revenue, and earn 109 USD PPP more profit (Table A.3).[28] These effects are all large and statistically significant: 8% of the placebo group mean for labour supply and roughly 35% for the other outcomes. The effect on profit is also large but less precisely estimated than the other outcomes ($p = 0.12$). The workshop also increases adoption of new business practices in non-farm enterprises, such as new/improved products and new customers/markets (Table A.3, column 4). In contrast, crop- and livestock-raising contribute little to the treatment effects on input expenditure and revenue, even though there are two growing seasons between treatment and endline. The fact that investment and revenue

[28]Breaking down effects by type of household economic activity is prespecified. All measures in Table A.3 are prespecified except returns to factors and profit. We code activity-specific measures as zeroes for households that do not engage in that activity to avoid sample selection problems. For example, we code non-farm enterprise investment and revenue as zero for households without non-farm enterprises. Hence treatment effects for the roughly half of the sample who own an enterprise are even larger.

Table 2: Treatment Effects on Economic Behaviour

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Expenditure	Revenue	Non-Land Assets	Consumption Expenditure
Asp&Plan	0.112***	26.8**	230**	22.2	260*	98**	142*
	(0.035)	(11.6)	(100)	(27.7)	(155)	(46)	(74)
	[.]	[0.069]	[0.069]	[0.127]	[0.069]	[0.069]	[0.069]
Cash	0.234***	27.2**	451***	44.8	465***	406***	322***
	(0.039)	(12.4)	(103)	(30.4)	(159)	(50)	(77)
	[.]	[0.012]	[0.001]	[0.036]	[0.003]	[0.001]	[0.001]
Combined	0.258***	9.0	653***	126.4***	546*	352***	232**
	(0.063)	(11.5)	(214)	(31.5)	(303)	(47)	(95)
	[.]	[0.096]	[0.004]	[0.001]	[0.030]	[0.001]	[0.012]
P: cash	0.003	0.972	0.040	0.503	0.230	0.000	0.032
= asp&plan	[.]	[0.526]	[0.073]	[0.433]	[0.208]	[0.001]	[0.073]
P: cash	0.717	0.127	0.357	0.022	0.801	0.292	0.367
= combined	[.]	[0.466]	[0.581]	[0.150]	[0.788]	[0.581]	[0.581]
P: asp&plan	0.004	0.118	0.007	0.001	0.217	0.000	0.327
= combined	[.]	[0.097]	[0.010]	[0.003]	[0.151]	[0.001]	[0.196]
P: cash	0.166	0.007	0.881	0.166	0.536	0.025	0.049
+ asp&plan	[.]	[0.046]	[0.416]	[0.143]	[0.332]	[0.066]	[0.081]
= combined							
Placebo mean	-0.000	525	857	640	2,101	1,529	3,796
# clusters	413	413	413	412	413	413	412
# obs	7,243	7,240	7,243	6,273	7,243	7,242	7,224

Notes: This table shows household-level treatment effects of the interventions on six prespecified economic outcomes and an index combining them. All currency values are measured in 2018 USD PPP. All outcomes except education expenditure are in annual terms. The outcome in column (1) is an inverse covariance-weighted average of the outcomes in columns (2)-(7), following Anderson (2008). The outcomes in columns (2)-(7) use the same definitions as in Table 1. Coefficients are from OLS regressions of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across all outcomes except the summary index are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. The number of observations varies slightly across columns because some respondents don't answer all questions, which also drops one small village.

effects are concentrated in the same non-farm enterprises is consistent with a causal chain from treatment-induced investment to revenue.

The workshop raises non-land asset value by 98 USD PPP: 6% of the placebo group mean (Table 2, column 6). The increase in asset stocks is mostly explained by small, non-lumpy items

– durable assets and cash savings (Tables A.6). Effects on livestock are driven by small livestock, not by lumpy purchases of large livestock that would be difficult to afford without a direct wealth infusion. There are changes in savings behaviour: respondents join more savings groups, used as commitment savings devices in this area (Gugerty, 2007), and make more contributions to them.

The workshop increases consumption expenditure by 142 USD PPP, 4% of the placebo mean (Table 2, column 7), typically interpreted as a rise in household welfare. This might be financed by the higher revenue generated by the treatment-induced rise in investment. The workshop raises both consumption and input expenditure, but it shifts the composition of spending from consumption toward investment. The placebo group's mean ratio of input expenditure to consumption is 0.22, while the ratio of the treatment effects on input expenditure to consumption is 1.6. Hence the marginal resources generated by treatment are allocated more to investment than consumption, relative to the non-treatment allocation. This is consistent with the workshop's emphasis on working and investing towards long term aspirations.

These results provide strong evidence that the aspirations and planning workshop substantially shifts economic investment and outcomes. The workshop shifts a summary index of the six aggregate measures by 0.11 standard deviations (Table 2, column 1). Effects on five of the six economic aggregates are statistically significant, even after adjusting for multiple hypothesis testing over the six outcomes, a more stringent adjustment than we prespecified. These are large effects for a 60-90-minute workshop occurring 17 months earlier.

5.2 Economic Effects of Cash Transfers

The cash transfer increases our summary index of the six economic aggregates by 0.23 standard deviations, roughly twice the aspirations and planning workshop's effect (Table 2, column 1). The relative magnitudes of the cash transfer and workshop effects vary substantially across the different economic aggregates. Both interventions have similar effects on annual household labour supply.[29] Given the large resource injection of the cash transfer, its effects on all pecuniary outcomes are unsurprisingly larger than the workshop: roughly double the size for input expenditure, education expenditure, and revenue; triple for consumption; and quadruple for assets, although not all differences are statistically significant. Effects are extremely similar to a similar study of the same programme in a different area (Egger et al., 2022).

5.3 Economic Effects of the Combined Intervention

The combined intervention increases all economic aggregates we study but does not have systematically larger effects than the cash transfer alone. In particular, it increases the economic index by

[29]Most cash transfer evaluations find non-negative labour supply effects (Banerjee et al., 2017).

0.26 standard deviations, only 0.02 standard deviations more than the cash transfer alone (Table 2, column 1). This is potentially surprising, as the aspirations and planning workshop alone increases this economic index by a substantially larger 0.11 standard deviations relative to the placebo. For five of the six aggregates, the combined-cash difference is not statistically significantly different to zero and is smaller than the substantial workshop-placebo difference (columns 2-3, 5-7).[30] We discuss potential explanations for these patterns in Section 6.4.

Education expenditure is the only outcome where the effect of adding the Asp&Plan workshop to the cash transfer significantly exceeds the effect of the cash transfer alone (Table 2, column 4). The combined arm increases education spending by 127 USD PPP: 20% of the placebo group mean, six times the effect of the workshop, and triple the effect of the cash transfer. This is driven by spending on school fees for both primary and secondary school-aged children (Table A.2). It is not accompanied by any shift in school enrolment or attendance (Table A.1). It might reflect spending on lumpy items like fees for boarding, private schools, or higher-quality public schools (Lucas and Mbiti, 2012), but we do not observe the type of school attended at endline.

5.4 Heterogeneous Treatment Effects of the Interventions

All our interventions produce broad-based economic improvements, as no intervention has treatment effects that vary substantially across different types of respondents. We summarise results of two analysis methods here and report details of the results and methods in Appendix A.3.

First, we estimate heterogeneous treatment effects on the summary economic index using standard treatment-interacted regressions across eight prespecified baseline measures – respondent age, marital status, education, aspirations, expectations, self-beliefs, household assets and household size – plus the economic index itself.[31] The estimated interaction effects are seldom large and the fraction of statistically significant estimates is no larger than would arise by chance. In particular, the workshop's effects do not vary by baseline wealth or by baseline values of the targeted psychological concepts: aspirations and expectations. They also do not vary by respondent age, household size, or marital status, even though our sample includes a wide range of demographics.[32]

Second, we test the null hypothesis of constant treatment effects on the economic index using

[30] We can also combine the three investment outcomes in columns 2-4 into a single, non-prespecified 'productive investment' index. The combined intervention has a larger effect on this index than the cash-only intervention: 0.23 standard deviations versus 0.17. But the confidence intervals overlap so we recommend against strong interpretation. The cash-combined difference in labour supply effects is substantial relative to the effect sizes but not statistically significant. This occurs because respondent labour supply is similar in the cash and combined groups, while non-respondent adult and child members' labour supply is lower in the combined group (Table A.5).

[31] We also show two non-prespecified dimensions of heterogeneity, discussed in Section 6.

[32] Consistent with the lack of heterogeneous effects by marital status, Mahmud et al. (2022) find few effects of the individual interventions on intrahousehold relationship quality, decision-making, or intimate partner violence in a subsample of married women in one area.

causal forests (Wager and Athey, 2018). This is a data-driven method to flexibly partition the sample into subsamples defined by values of the nine baseline measures listed above and estimate treatment effects within each subsample. This approach estimates heterogeneous treatment effects simultaneously by all nine measures and does not impose a specific functional form. We do not reject the null hypothesis that treatment effects are equal across all subsamples.

6 Mechanisms

In this section, we provide evidence that aspirations and expectations are the most plausible mechanism for the economic effects of the aspirations and planning workshop and the limited effects of adding the intervention to cash. First, we show the workshop increases aspirations and expectations but has no effects on the other candidate mechanisms. Second, we rule out some alternative non-psychological mechanisms. Third, we present a simple conceptual framework to define aspirations and expectations in terms of economic concepts and show theoretically how they might affect investment and wealth. Finally, we show that the cash and combined interventions have similar effects on all psychological mechanisms, including substantial positive effects on aspirations and expectations; test possible explanations for this finding; and show how it might explain their similar effects on economic outcomes.

6.1 Effects of the Asp&Plan Workshop on Psychological Mechanisms

Table 3 shows effects of the workshop on candidate mechanisms. We code all measures so that higher values are theoretically associated with higher investment: e.g. we report effects on mental health (the Z score on a depression scale multiplied by negative one). We report sharpened q-values adjusting for multiple testing across all mechanisms. This is conservative because we prespecified time and risk preferences and mental health as unlikely mechanisms that we measured only for the purpose of ruling them out.

Aspirations and Expectations: The aspirations and planning workshop increases an index of aspirations for the future by 0.092 standard deviations relative to the placebo group (Table 3, column 1). This effect is broad-based: it is non-negative at all quantiles, with no clear pattern of larger effects at higher or lower quantiles, and the average effect does not vary substantially by baseline aspirations or other baseline characteristics (Figures A.7 and A.9). The treatment effect on aspirations is driven by a positive and significant increase in aspirations for children's education; there are positive but imprecisely estimated increases in aspirations for assets and income (Table A.7). The magnitude of changes is modest: for example, respondents aspire to have 265 USD PPP more assets and 127 USD PPP more annual income in ten years (Table A.7). Abstracting from inflation, these are respectively double the treatment effect on assets at endline

(Table A.6) and similar to the treatment effect on profits (Table A.3). We focus on effects on the index, which we view as a measure of a general aspirational mindset. Differences across domains of aspirations may be due to differential effects of the workshop on different types of aspirations or the fact that the measure of education aspirations covers a period further into the future.

The workshop also increases an index of expectations for participants' future outcomes by 0.091 standard deviations relative to the placebo group (Table 3, column 2). The increases across domains, patterns across quantiles and lack of heterogeneous effects mirror those for aspirations. The similarity of aspirations and expectations effects suggests that participants view their higher aspirations as attainable. The aspirations and expectations effects are statistically significant at the 1 and 5% levels respectively. They remain significant when we adjust for multiple hypothesis testing across all eight mechanisms, while no effect on any other mechanism is large or statistically significant.

The workshop also increases aspirations and expectations relative to participants' beliefs about their current economic position. Recall from Section 3 that we measure participants' beliefs about their current assets and income before measuring their aspirations. We construct an index of aspirations minus current perceived position using these asset and income measures, along with participants' aspired education level for their child minus their own education level. The treatment effect on this index is only slightly smaller than on aspirations itself, and a similar pattern holds for expectations minus current perceived position. This shows that treatment raised aspirations and expectations for their economic futures, rather than simply changing participants' beliefs about their current economic position.

Self-Beliefs: Treatment has no effect on the index of three psychological scales capturing self-beliefs (Table 3, column 3) or on any of the individual scales (Table A.8). This is in line with studies in psychology, which find little empirical evidence that the individual psychological exercises change self-beliefs (Conroy and Hagger, 2018; Kwasnicka et al., 2013). While self-efficacy or similar traits can be altered, this is largely documented with more intensive interventions (Carlana et al., 2022; Ghosal et al., 2020; McKelway, 2021). Evidence on growth mindset suggests it mainly responds to specific interventions different from those we implement (Yeager and Dweck, 2020).

Information Acquisition, Beliefs about Returns, Mimicry or Recall: We find little evidence that the workshop changes beliefs about returns to specific activities. There is no effect on beliefs about returns to labour on the farm or to investment in university education, which we measured because the videos contain characters making these investments (Table A.9). This is unsurprising: we minimise the potential for learning about the returns to activities from the workshop by comparing the aspirations and planning workshop group to the placebo group, who receive the same images and descriptions of economic activities. We also find no effect on beliefs

Table 3: Treatment Effects on Psychological Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aspir-ations Index	Expect-ations Index	Self-belief Index	Returns Index	Discount Factor	No Present Bias	Risk-taking	Mental Health Z-score
Asp&Plan	0.092***	0.091**	0.006	-0.011	-0.006	-0.023	-0.018	-0.015
	(0.035)	(0.039)	(0.046)	(0.037)	(0.011)	(0.016)	(0.038)	(0.035)
	[0.079]	[0.079]	[1.000]	[1.000]	[1.000]	[0.428]	[1.000]	[1.000]
Cash	0.130***	0.178***	-0.053	-0.002	-0.002	-0.015	-0.014	0.086**
	(0.036)	(0.038)	(0.042)	(0.038)	(0.011)	(0.016)	(0.036)	(0.034)
	[0.002]	[0.001]	[0.344]	[0.907]	[0.907]	[0.526]	[0.854]	[0.027]
Combined	0.178***	0.134***	0.025	-0.002	-0.003	-0.028*	-0.030	0.044
	(0.040)	(0.043)	(0.044)	(0.036)	(0.011)	(0.017)	(0.033)	(0.035)
	[0.001]	[0.006]	[0.668]	[0.908]	[0.788]	[0.235]	[0.594]	[0.339]
P: cash	0.324	0.041	0.198	0.830	0.728	0.617	0.927	0.006
= asp&plan	[0.948]	[0.170]	[0.657]	[1.000]	[1.000]	[1.000]	[1.000]	[0.052]
P: cash	0.258	0.331	0.043	0.997	0.935	0.402	0.696	0.207
= combined	[1.000]	[1.000]	[0.528]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
P: asp&plan	0.027	0.318	0.687	0.813	0.781	0.768	0.777	0.097
= combined	[0.277]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.515]
P: cash	0.398	0.019	0.224	0.825	0.728	0.652	0.958	0.581
+ asp&plan	[1.000]	[0.180]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
= combined								
Placebo mean	0.000	0.000	0.000	0.000	0.698	0.753	0.000	-0.000
# clusters	413	413	413	413	413	413	413	413
# obs	7,232	7,233	7,221	7,110	7,243	7,243	7,170	7,213

Notes: This table shows treatment effects of the interventions on psychological outcomes that might explain the treatment effects on economic outcomes. The aspirations index and psychological measures are defined in the footnote below Figure 1. Most outcomes are inverse covariance-weighted averages of multiple measures so the treatment effects are in standard deviation units, following Anderson (2008). Measures are defined in detail in Section 3.1. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For psychological outcomes where the order in which questions appear in the survey are randomised, a set of order indicator variables are also included as additional controls. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. The number of observations varies slightly across columns because some respondents don't answer all questions, which also drops one small village.

about returns to fertiliser, an input not featured in the videos, nor on an index of all three beliefs (Table 3, column 4). We don't measure beliefs about returns to all possible investment activities, given the number of activities featured in the videos.[33]

We also test if respondents simply copy specific activities depicted in the aspirations and

[33]We also measured beliefs about the profits from being a tailor at baseline. Treatment effects on investment do not vary by baseline beliefs about returns to tailoring, providing some evidence against these beliefs driving investment behaviour. We dropped the measure at endline because many respondents struggled to answer the question as they were unfamiliar with revenue and cost in this activity.

planning and placebo videos. The Asp&Plan workshop does not increase an index of five dummy variables capturing if respondents mimic activities shown in the videos: weaving baskets, keeping savings in jar, taking a sewing class, training as a teacher, or growing vegetables for market sale (Table A.10, column 6). This is perhaps because the activities were common – 28% of placebo group respondents engage in at least one.

Finally, we examine if people are more likely to recall or pay attention to information about activities in the workshop because it is more entertaining (Hanna et al., 2014; La Ferrara, 2016). In both the aspirations and planning and placebo videos, we include information about returns to education in neighbouring counties of Western Kenya, taken from Ozier (2018). The workshop does not improve recall of this information relative to the placebo (Table A.10, column 5). Thus, while we cannot measure beliefs or recall over all information in the video, we find no direct evidence for a framework based on information acquisition, mimicry of particular activities or improved recall of information.

Time and Risk Preferences: The workshop has negligible effects on risk preferences and on two proxies for patience over monetary payments: the discount factor and an indicator for not being present biased (Table 3, columns 5-7). We prespecified that we did not expect changes in these preferences. Other light-touch interventions also find no effects on these conventional measures of time preferences (John and Orkin, 2022).

Mental Health: The workshop has a very small, non-significant effect of 0.015 standard deviations on mental health (Table 3, column 8) and near-zero effects on indicators for whether an individual meets criteria for clinical depression (Table A.8).[34] We prespecified changes in mental health as unlikely, as the workshop is shorter than most therapies, which take 6 to 20 sessions, and omits many key elements of common therapies (Cuijpers et al., 2013). The workshop's effects on aspirations and economic outcomes do not vary by baseline mental health (Figure A.7) and effects on mental health do not vary by other baseline characteristics (Figure A.8).

6.2 Ruling Out Alternative Non-Psychological Mechanisms

Experimenter Demand: Our measures are based on self-reported data, as is common in studies in poor rural settings with very little administrative data. Participants in the workshop may give answers that they believe the surveyors want to hear, perhaps reporting choices consistent with what they reported in the exercise. Three pieces of evidence suggest this does not drive effects.

First, we collect two prespecified objective measures of outcomes where we also collect self-

[34]The measure of depression captures symptoms of clinical depression like poor sleep, reduced appetite and feelings of hopelessness. It would be unlikely to pick up changes in general wellbeing, such as those due to small improvements in living conditions.

reported data. If the treatment effects of the Asp&Plan workshop were driven by socially desirable reporting, we would see larger treatment effects on the self-reported measures than the verified measures as households would be inflating self-reported measures. Similarly, we would see larger differences between the combined and cash arms on self-reported measures than verified measures.

We do not find evidence consistent with experimenter demand using our first measure, of housing expenditure and quality. Column 1 of Table A.10 reports on self-reported measures of housing expenditure (in USD PPP) and Column 2 on enumerator ratings of the expense of housing materials on a standardised scale from one to four that rates more expensive materials higher (Arias and De Vos, 1996). The workshop has no effect on either self-reported housing expenditure nor verified housing quality, even though one household in the video is shown buying a higher-quality roof so respondents might think reporting housing expenditure was socially desirable. The results in the cash and combined arms also do not suggest experimenter demand: both interventions have large positive effects on both self-reported housing expenditure and verified housing quality. The difference between the cash and combined arms is negligible for both the self-reported and the verified measure.

Similarly, we do not find evidence consistent with experimenter demand using our second measure, of small asset ownership. Column 4 captures enumerators' count of the number of seven easy-to-observe asset types that the household owns, which was done at the end of the survey and not preannounced. Column 3 reports on the self-reported number of these assets from earlier in the survey. The treatment effect of the workshop are consistently zero on both self-reported and enumerator-observed measures of these assets.[35] Again, there is no sign the combined arm inflate their reports: enumerators observe increases in the number of assets households owned by households in the cash and combined arms; both arms report similarly on these increases. Table A.11 shows treatment effects for each of the seven assets that households report owning (Panel A) and that households are verified as owning (Panel B). Again, treatment effects are highly consistent across self-reported and verified measures for all three treatment groups.

Our second piece of evidence against experimenter demand effects comes from Section 6.1, where we showed workshop participants are not more likely to report doing specific activities depicted in the video. Third, the Asp&Plan workshop does not increase self-reported education spending for secondary school children (Table A.2) and no intervention increases school enrolment or attendance (Table A.1), even though these are socially desirable behaviours depicted in the videos.

Spillovers: Our village-level randomisation reduces spillover effects relative to designs that randomise within villages, schools, or even within community/social groups. We also find people

[35]Note: the workshop increases the total self-reported value of *all* assets including livestock, savings and durables (Table 2, column 6). But it does *not* increase self-reports of the number of the seven specific asset types we focus on for this measure.

talk very little about the issues raised in the workshop, and even less across villages. People talk infrequently with others about their goals and challenges – fewer than two on average in the last 12 months – and this discussion is concentrated within villages (Garlick et al., 2022). We test for within-village spillovers using a random sample of households in Asp&Plan workshop and placebo villages who are too wealthy to receive the cash transfer on GiveDirectly's criteria. Spillover effects of the workshop on the main psychological and economic outcomes are mostly small and none are statistically significant after correcting for multiple hypothesis testing (Tables A.12 and A.13). The lack of within-village spillovers means that spillovers onto the placebo group, who live farther away in separate villages, are unlikely. However, we view these result with some caution as untreated respondents within the same village may be affected by knowledge that others received interventions.

Group/facilitator effects: As discussed in Section 4.1, the placebo-controlled design means that interaction with outsiders or meeting with a group should not drive the workshop's effects. We also see no evidence that group delivery or composition drives results. Group size is balanced across treatment arms. Under half of respondents report at endline that they still talk to their group members. We also find no heterogeneity in effects on the economic or aspirations index when we compare respondents who receive the workshop in groups to the sample of recipients who receive the intervention individually because they miss their group meeting (Figure A.7). Our main findings are robust to using facilitator fixed effects.

Hawthorne or John Henry Effects: In experimental settings where the control group are aware others receive a different treatment, they could change reporting or behaviour because they are disappointed or believe the treatment group have received information that advantages them (List, 2011). Alternatively, the treatment group may believe they have particular attributes which have led to their selection. Our village-level design largely rules out potential effects from knowledge that others had received a different intervention.

6.3 A Conceptual Framework Linking Psychological and Economic Outcomes

We propose a simple framework to show how the aspirations and planning workshop can activate a causal chain from aspirations, which proxy for reference points for future consumption, to investment and other economic outcomes. We also discuss how expectations can enter into this framework.

Our framework is one of reference-dependent utility, where people receive an extra utility gain, an "aspirational payoff", from reaching or exceeding their reference point for consumption. In each period t, agents derive utility from leisure l_t and consumption c_t relative to a reference point h_t, akin to aspirations, which they take as given:

$$u(c_t,l_t)=v(c_t,l_t)+z(c_t-h_t), \tag{2}$$

This follows existing models of aspirations (Dalton et al., 2016; Genicot and Ray, 2017; Lybbert and Wydick, 2018).[36] We assume v is increasing and concave in both arguments, as is standard. We also assume people gain utility at a decreasing rate from exceeding their reference point and lose utility from missing it ($z'>0$, $z''<0$, $\text{sign}\{z(c-h)\}=\text{sign}\{c-h\}$).[37] We assume households maximize intertemporal utility that is additively separable through time with discount factor δ.

People enter period t with asset holdings a_t and receive revenue y_t, which they allocate between consumption and capital investment k_t. They allocate their time endowment T between leisure and labour e_t. Their investments generate revenue $y_{t+1}=f(k_t,e_t)$ in the next period. Assets depreciate at rate γ each period. We assume revenue is increasing and concave in both arguments.

This problem has choice variables consumption and leisure, yielding first-order conditions:

$$v_{c_t}+z_{c_t}=\delta\cdot(1+f_{k_t}-\gamma)\cdot\left(v_{c_{t+1}}+z_{c_{t+1}}\right) \tag{3}$$

$$v_{l_t}=\delta\cdot f_{e_t}\cdot(v_{c_{t+1}}+z_{c_{t+1}}). \tag{4}$$

The first condition is an Euler equation: people set the marginal utility of current consumption equal to the discounted marginal utility of future consumption arising from current capital investment. The second condition is the labour-leisure trade-off: people set the marginal utility of current leisure equal to the discounted marginal utility of future consumption arising from current work. In both conditions, the marginal utility of consumption includes a term $z_{c_{t+1}}$ capturing reference dependence.

This framework shows how the Asp&Plan workshop, by raising aspirations for the future h_{t+1}, can generate the estimated treatment effects on economic outcomes. Higher h_{t+1} increases the marginal utility of future consumption $v_{c_{t+1}}+z_{c_{t+1}}$. Formally, this occurs because z is a concave function of c_t-h_t. Intuitively, this occurs because raising h_{t+1} shifts agents farther below their consumption reference point, making gains in future consumption more valuable. The higher marginal utility of future consumption motivates people to invest now to afford higher future consumption: they raise current capital investment to maintain condition (3) and raise current labour supply to maintain condition (4). This matches the positive treatment effects on input expenditure and on labour supply that we observe in the experiment. This causes higher future revenue and asset values through $f(k_t,e_t)$, matching the positive treatment effects on revenue and assets we observe. The framework does not explicitly model investment in or revenue from education. But, informally, the workshop will increase current education expenditure for the same reasons that it increases current capital investment and labour supply, matching the positive

[36] We assume that aspirations are malleable. But we follow the literature by assuming that agents cannot choose their reference point, otherwise they would set $h_t=0$ to maximise utility (Lybbert and Wydick, 2018).

[37] Other models use slightly different assumptions about the shape of z to study different behaviour, including loss aversion over gambles (DellaVigna et al., 2017; Kahneman and Tversky, 1979). These behaviours are less relevant for our study so we assume z is concave and continuous for tractability.

effects on investment in younger children's education.

The effect of an increase in aspirations on consumption at the time of the endline survey is theoretically ambiguous. There is an intertemporal substitution effect: there is more incentive to invest now and consume in the future, so the time path of consumption will steepen and consumption in periods close to t will fall. But there is also a wealth effect, as the rise in current investment and labour supply increases future assets. Which of these effects dominates in any specific period depends on the parameterisation of the model, as in Deaton (1992). The positive treatment effect we estimate on consumption after 17 months suggests a large role for the wealth effect.

Expectations can enter this framework in two different ways. First, expectations may be another proxy for the reference points, as in Kőszegi and Rabin (2006, 2007). Expectations then have a dual role: they determine economic choices via reference points, and also reflect beliefs about the outcomes of economic choices, leading to complex equilibrium concepts when expectations are formed endogenously. Second, aspirations might proxy for reference points, be affected by the workshop, and drive changes in economic choices and outcomes, while expectations capture beliefs about the outcomes of these changes in economic choices. In the first interpretation, expectations respond directly to the workshop and then affect economic outcomes. In the second interpretation, expectations respond indirectly to the workshop via effects on economic outcomes. The close empirical relationship between aspirations and expectations in our data is consistent with both interpretations, so we do not argue that either one is more plausible than the other.

In Appendix E, we show how other psychological mechanisms could enter this framework and show that increases in patience, self-beliefs, or beliefs about returns could theoretically produce the same economic effects. Our rich data on psychological mechanisms allows us to show that the workshop does not affect outcomes through these alternative mechanisms, which we could not have concluded using only data on the economic effects.

6.4 Effects of the Cash and Combined Intervention on Psychological Mechanisms

What might explain the similar economic effects of the combined cash-and-workshop intervention and cash alone, despite the substantial effects of the aspirations and planning workshop by itself?

Treatment effects on mechanisms suggest an important role for aspirations and expectations. The combined intervention and cash transfer alone produce similar effects of 0.13 - 0.18 standard deviations on aspirations and expectations, while the workshop raises both aspirations and expectations by 0.09 SDs relative to the placebo (Table 3, columns 1-2).[38] In contrast, columns 3-8 show

[38]These increases in aspirations and expectations do not just occur because the cash transfers raise recipients' current wealth. To show this, we estimate treatment effects on aspirations and expectations minus respondents' beliefs about their current economic position, measures described in Sections 3.2 and 6.1. Treatment effects on these outcomes are still positive and substantial: cash and combined raise aspirations less current position by

that both the cash and combined interventions have similar, near-zero effects on other psychological mechanisms, other than modest positive effects on mental health, in line with Ridley et al. (2020).

At first sight, this may seem to clash with the conceptual framework: shifting aspirations through the Asp&Plan workshop should have resulted in an additional effect on investment beyond the lifetime wealth effect from the cash transfer. However, once we allow the reference point h_{t+1} to endogenously rise in response to the cash transfer, this pattern of results is consistent with our conceptual framework and the decision rules implied by (3) and (4).[39] The Asp&Plan workshop activates a causal chain from reference points, proxied by aspirations and expectations, to investment. It activates this chain by itself but not when added to a cash transfer, potentially because the cash transfer itself activates the aspirations-to-investment chain. The treatment effects suggest that the workshop and cash transfer have strongly concave or substituteable effects on aspirations, so the aspiration increase may even plateau, seemingly crowding out the effect of the workshop in our results for the combined intervention.

The positive effect of wealth shocks on aspirations is a novel finding, which advances work on reference point formation (e.g. Kőszegi 2010) and shows that aspirations can respond relatively quickly to changing economic circumstances as in Dalton et al. (2016), rather than depending only on long-term circumstances or culture (Genicot and Ray, 2017; Lybbert and Wydick, 2018).

We evaluate two possible explanations for the pattern of apparent 'aspirational crowd out.' The first explanation is a *targeting* effect: people's reference points might simply be less sensitive to external shocks if they are wealthier. To test this, we add to our treatment effects model in equation (1) a proxy for baseline economic resources and interactions between this proxy and the treatment indicators. Under the targeting explanation, the workshop should be less effective for households with more resources, so the interaction between resources and the workshop should be negative for most economic outcomes. Instead, this interaction is essentially zero for all economic outcomes and all three resource proxies we use: the value of all assets, assets excluding the (difficult to measure) value of land and housing, and annual consumption (Table A.14).[40] Given this result, we reject the targeting explanation.

The second possible explanation is a *windfall* effect: the windfall or unanticipated nature of the cash transfer might itself increase reference points, proxied by aspirations, more than an

respectively 0.10 and 0.12 standard deviations (both with standard errors 0.03).

[39]Dalton et al. (2016) consider models where aspirations are an endogenously increasing function of wealth and agents either are or are not aware of this relationship. When agents are aware, it implies more complex first-order conditions but this not affect the qualitative predictions that are relevant for this paper.

[40]There is enough variation in baseline economic resources that this exercise does not rely on extrapolation outside the support of the data. The 2,237 USD PPP cash transfer corresponds to 0.18 standard deviations (SDs) of baseline assets, 1.5 SDs of baseline non-land-non-housing assets, and 0.84 SDs of baseline consumption. We obtain a similar result using spline models that allow more flexible heterogeneous treatment effects.

equivalent amount of anticipated wealth. This could reduce the workshop's effects when combined with the cash transfer, for example due to concavity in the extent to which aspirations can be boosted in each period. The targeting and windfall explanations differ because the latter concerns unanticipated, rapid increases in wealth, while the former concerns wealth in general, which might be anticipated and gradually accumulated. The two explanations have different policy implications: targeting informs whether poorer or less poor groups of people should be offered the workshop; windfall informs if and how the workshop should be offered in the presence of (large) cash transfers or other wealth shocks.

We find some evidence consistent with a windfall explanation using two tests based on responses to windfall versus other wealth. The first test compares the share of total expenditure allocated to investment between the placebo and cash groups. A standard life-cycle model predicts that this share will be lower in the cash group, as unanticipated wealth raises lifetime income and hence present consumption by more than a change in anticipated wealth would (Deaton, 1992). Instead, we find the opposite pattern. The cash treatment effect on this share is 3.1 percentage points with standard error 0.6, compared to a placebo group mean of 19.2% (Table A.15, column 1). This finding is not due to differences in total expenditure between the groups: controlling for total expenditure, the cash group invests 1.8 percentage points more of their expenditure than the placebo group (column 2); and including expenditure $\times$ treatment interactions shows that the cash group's higher investment share does not substantially vary by total expenditure (column 3).[41] This pattern is consistent with a reference-dependent utility model in which windfall wealth increases the reference point for future consumption, leading to an increase in the share of expenditure invested.

The second test compares the relationship between aspirations and wealth for windfall versus non-windfall wealth. Figure A.5 compares the estimated effect of the cash transfer on aspirations to estimates from regressing the aspirations index on three different wealth proxies using placebo group data with different sets of controls. Under the windfall explanation, the effect of one dollar of cash transfer on aspirations (column 4) would be larger than a change in aspirations associated with a one dollar increase in naturally occurring wealth (columns 1-3). This pattern clearly holds for one wealth proxy: the total value of all assets. This is the most comprehensive wealth proxy we observe but the most difficult to measure. Evidence is mixed for the other two proxies: the values of consumption and non-land assets. Figure A.6 shows similar patterns for expectations.

We conclude that the Asp&Plan workshop's substantial economic effects by itself and modest economic effects when added to a cash transfer most likely occur because the workshop and cash

[41]The regressions that control for endline expenditure might have endogeneity problems but the results are robust to including or excluding the prespecified baseline covariates and to trimming the tails of total expenditure to reduce sensitivity to possible outliers.

transfer both shift the same mechanism: reference points, proxied by aspirations or expectations. This pattern does not occur because the workshop has smaller effects on wealthier households. It might occur because windfall resources raise aspirations and expectations and hence raise investment more than a standard wealth effect would predict, seemingly crowding out the aspiration-promoting effect of the workshop itself. Our findings raise the possibility that cash transfers can shift economic outcomes through both conventional wealth effects and behavioural effects.

We conclude by noting one more pattern. The combined intervention, relative to cash, has a much larger effect on education spending (Table 2, column 4) and a substantially but not statistically significantly larger effect on education aspirations (Table A.7). Education aspirations and expenditure are also robustly positively correlated in the placebo group. This raises the possibility of domain-specific aspirations-investment relationships, as suggested by Genicot and Ray (2020). We leave a full analysis for future work.

7 Intervention Costs and Benefit-Cost Ratios

The Asp&Plan workshop has substantial benefits when offered on its own. When combined with a large cash transfer, it has few effects relative to the cash alone, although it slightly changes the mix of household expenditure across education, consumption and assets. This section evaluates whether the Asp&Plan workshop yields positive returns by the time of the endline by presenting data on the costs of the intervention and results of one method of comparing benefits to costs.

Intervention Costs: Table D.1 reports the actual intervention costs collected from our delivery of the workshop and GiveDirectly's delivery of the cash transfer, calculated following J-PAL (2016) guidelines. Costs are per participant, calculated as the total cost divided by the number of people offered treatment, in 2018 USD PPP, for consistency with the intention-to-treat estimates we use to evaluate benefits. The average variable cost of just workshop *delivery* was a relatively low 54 USD PPP, including staff time, travel and the tablets (Sub-Panel A1). This cost could be lowered even further by delivering the workshop as part of an existing field operation, such as activities of community health or agricultural extension workers.[42]

We incurred additional costs running the workshop for our trial that could be reduced or avoided if the intervention were delivered as part of an existing programme or at larger scale. The average variable *programme* cost was 218 USD PPP, which covered censusing participants to target the programme at poor households, as well as indirect costs for running a field operation (Sub-Panel A2). The average fixed cost of workshop *development* was 80 USD PPP per person offered the treatment (Sub-Panel A3). Delivering workshops to more households if the intervention were scaled would

[42] Although the results from the combined arm suggest that it is only useful to add the workshop to activities that are unlikely to shift aspirations or planning skills on their own.

lower fixed costs per beneficiary. Adding together these fixed and variable costs, the workshop has an average total cost per participant of 353 USD PPP in our study (bottom row of Panel A, Table D.1).

Benefit-Cost Ratios: We compare intervention costs and benefits to evaluate if the Asp&Plan workshop has a positive return. We define benefits as improvements in living standards, proxied by higher household consumption, following the standard approach from Deaton (1997). To roughly estimate increases in consumption, we follow similar principles to Bandiera et al. (2017) and Banerjee et al. (2015) who also study programmes targeted at poor households aimed at improving living standards. Panel B of Table D.1 shows our measure of accumulated benefits from intervention delivery to endline 17 months later. This is the sum of four treatment effects – on consumption expenditure, education expenditure, land and housing expenditure, and the value of non-land non-housing assets.[43] The first three treatment effects capture increases in spending due to the interventions. Effects on asset holdings at the endline proxy for treatment effects of the intervention on future consumption, as the additional assets can be sold to fund future consumption. This is a strong assumption but some assumption is unavoidable unless we can observe participants' lifetime consumption. We do not attempt a full benefit-cost analysis that accounts for potential spillover effects or the value of alternative uses of money.

Strikingly, there is a relatively high impact and benefit-cost ratio from this modest one-session workshop. Using this benefit measure, and at the study's scale, the ratio of benefits to average total costs of the aspirations and planning workshop is 96% i.e. it nearly pays for itself after 17 months, even when using the highest possible measure of intervention costs (Panel C, Table D.1). The benefit-cost ratio using just the variable costs of delivering the workshop is a massive 627%.

Using a similar approach, we can compare the benefit-cost ratio of the cash and combined intervention. The combined intervention has marginally lower ratio of benefits to total costs (47%) than the cash transfer alone (56%), as the benefits are smaller while its costs are higher, although the difference in benefit-cost ratios is not statistically significant (Panel D, Column 2, Table D.1).[44] From a policy evaluation perspective, these results suggest there is little justification for adding the workshop to cash using this one measure of programme benefit 17 months after intervention. However, this calculation makes the strong assumption that the benefits from expenditure on assets and education equal the amount spent on them. Some assumption about the benefits is unavoidable unless we can observe participants' lifetime outcomes from spending on assets and

[43]We scale the consumption and education effects from annual figures to cover the individual-specific period from the interventions to endline. We use effects on a flow measure of expenditure on land and housing, asked directly about the period from the intervention to endline.

[44]We calculate the cost of the cash transfer excluding the cost of the placebo intervention. If the placebo intervention delivers any benefits, our approach attributes extra benefits to the cash transfer and hence understates the cost-effectiveness of the combined intervention relative to the cash transfer.

education, but alternative assumptions might make the combined arm more attractive.

We do not formally compare the benefit-cost ratio of the aspirations and planning workshop to the cash transfer. The cash transfer programme was chosen to be combined with the workshop because it had been shown to meaningfully improve living standards (Egger et al., 2022; Haushofer and Shapiro, 2016) and we sought to test the effects of the workshop on its own and in a population whose living standards had been meaningfully improved. But this meant cash transfer costs were fixed by GiveDirectly and the cash transfer costs about six times the workshop (2,149 USD PPP vs 353 USD PPP). This prevents a precise benchmarking exercise, where the benefits of a non-cash intervention are compared to a cash transfer of the same cost as the non-cash intervention McIntosh and Zeitlin (2021, 2022). If returns to cash transfers are non-linear, the benefit-cost ratios of different sizes of transfer will differ, so the benefit-cost ratio of this transfer is not a guide to that of a transfer costing the same amount as the workshop.[45]

8 Conclusion

We study the role of material and psychological constraints to investment and asset accumulation for people living in poverty in Western Kenya. We provide empirical microfoundations for the idea that people living in poverty may lack opportunities to build their "capacity to aspire" (Appadurai, 2013): to set higher aspirations to improve their socioeconomic position and plan to achieve these goals. The idea is important because psychological constraints could be one channel through which adverse historical conditions of discrimination and segregation persist and perpetuate contemporary poverty (Durlauf, 1996). We provide compelling evidence that aspirational capacity can be easily built at low cost: a short, scalable workshop targeted at aspirations and long-term planning has large effects on economic outcomes 17 months later. The workshop teaches people to set higher aspirations and medium-term goals; plan concrete, immediate steps to begin working toward those goals; and anticipate obstacles. In a cluster-randomised, placebo-controlled trial in 415 villages, we find the workshop increases recipients' economic investment and downstream outcomes: they work and invest more, leading to higher revenue, asset wealth, and consumption. We show the workshop likely works by increasing participants' aspirations and expectations: the long-term goals they choose and aim for, and their beliefs about what future is possible.

The average gain in consumption and assets by the time of the endline roughly equals the average cost of the workshop. This finding highlights that such interventions should be seriously considered as a tool for poverty reduction. There are potential market failures in the

[45]Existing research using randomised variation in transfer sizes shows monotonic transfer-consumption relationships but does not test for concavity (Blattman et al., 2013; Haushofer and Shapiro, 2016; McIntosh and Zeitlin, 2021). This may differ for outcomes other than consumption, on which we focus. For example, Baird et al. (2011) show the effect of cash transfers on school enrolment does not scale linearly with transfer size.

building of aspirational capacities. For example, there is no market where good role models can be compensated for the aspirational benefits they produce and institutions like schools or workplaces may not have incentive to develop these capacities. Further work could study how workshops like this work at larger scales and in different contexts and whether changes persist.

We find a complex set of results when we combine the workshop and cash transfer in another experimental arm. We find few differences between the effects of the combined intervention and the effects of the cash transfer alone. This might occur because the cash transfer alone raises participants' aspirations, crowding out the aspiration- and investment-promoting effect that the workshop delivers by itself. Such windfall effects may occur partly because the cash transfer is so large and delivering it in a lump sum enhances recipients' beliefs about what they can achieve. Future research could consider how transfer size and structure affects aspirational as well as economic benefits. However, our work highlights that existing anti-poverty interventions can shift economic outcomes through both conventional wealth effects and behavioural effects.

Our findings can also inform future work on psychological poverty traps. Recent work models 'aspirational poverty traps,' in which poverty lowers aspirations, which in turn lowers investment, and entrenches poverty (Dalton et al., 2016; Genicot and Ray, 2020; Lybbert and Wydick, 2018). Our results do not provide a direct test of these models but we do find evidence consistent with positive wealth-aspirations and aspirations-investment relationships. To the extent that some people face aspirational poverty traps, our results suggest that the low aspirations component of the trap might be addressed either through targeted interventions like our workshop or simply improving economic conditions.

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A Additional Results

A.1 Additional Treatment Effects

As we prespecified, we adjust for multiple hypothesis testing over tests that use components of the same prespecified aggregate or concept. This applies to the two components of age-group-specific education expenditure in Table A.2; input expenditure and hired labour in Table A.3, the four components of non-land assets in Table A.6, the three aspirations measures in Table A.7, the three self-beliefs in Table A.8, the two depression indicators in Table A.8, the beliefs about returns to specific inputs in Table A.9. We also adjust for multiple hypothesis testing across the six economic aggregates in each of Tables A.12, A.13, and A.14 to be consistent with Table 2. We do not adjust across all measures in a table if they are not components of the same prespecified aggregate.

Table A.1: Treatment Effects on Individual-level Education Participation and Enrolment

	(1)	(2)	(3)	(4)
	Education Participation		Education Enrolment	
	6-13	14-20	6-13	14-20
Asp&Plan	0.004	0.005	0.002	0.013
	(0.013)	(0.017)	(0.006)	(0.013)
	[1.000]	[1.000]	[1.000]	[1.000]
Cash	-0.004	0.006	-0.013**	0.013
	(0.012)	(0.016)	(0.006)	(0.013)
	[0.577]	[1.000]	[0.088]	[1.000]
Combined	-0.009	-0.006	-0.009	0.011
	(0.012)	(0.015)	(0.007)	(0.013)
	[0.553]	[1.000]	[0.553]	[1.000]
P: cash = asp&plan	0.573	0.975	0.021	0.961
	[0.402]	[1.000]	[0.044]	[1.000]
P: cash = combined	0.727	0.457	0.549	0.830
	[1.000]	[1.000]	[1.000]	[1.000]
P: asp&plan = combined	0.371	0.538	0.088	0.874
	[0.228]	[1.000]	[0.215]	[1.000]
P: cash + asp&plan	0.652	0.477	0.823	0.399
= combined	[1.000]	[0.913]	[1.000]	[0.913]
Placebo mean	0.892	0.679	0.977	0.753
# clusters	412	409	412	409
# obs	11,913	8,404	12,316	8,813

Notes: All variables are at the individual level, with one observation for each household member aged 6-20. Education participation is defined as the share of days of schools attended out of the last 5, coded as zero for non-enrolled students. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent, age, gender and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely each of the education measures for each age group, are shown in brackets.

Table A.2: Treatment Effects on Individual-level Education Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
	Education Expenditure		Fee Expenditure		Non-fee Expenditure	
Household members aged:	6-13	14-20	6-13	14-20	6-13	14-20
Asp&Plan	9.03*	2.3	7.67*	2.5	1.01	-1.55
	(5.01)	(14.8)	(3.95)	(13.2)	(1.56)	(3.75)
	[.]	[.]	[0.118]	[1.000]	[0.350]	[1.000]
Cash	9.56**	37.6**	7.50*	29.4*	2.15*	7.15*
	(4.66)	(18.8)	(3.86)	(16.6)	(1.29)	(4.17)
	[.]	[.]	[0.107]	[0.096]	[0.107]	[0.096]
Combined	13.29**	69.6***	11.18**	62.7***	1.74	7.76**
	(5.56)	(17.7)	(4.51)	(15.8)	(1.44)	(3.81)
	[.]	[.]	[0.029]	[0.001]	[0.130]	[0.022]
P: cash = asp&plan	0.923	0.063	0.968	0.105	0.483	0.054
	[.]	[.]	[1.000]	[0.118]	[1.000]	[0.118]
P: cash = combined	0.502	0.130	0.411	0.075	0.785	0.888
	[.]	[.]	[1.000]	[0.177]	[1.000]	[0.799]
P: asp&plan = combined	0.467	0.000	0.446	0.000	0.660	0.021
	[.]	[.]	[1.000]	[0.001]	[1.000]	[0.011]
P: cash + asp&plan	0.458	0.236	0.485	0.168	0.498	0.700
= combined	[.]	[.]	[0.993]	[0.507]	[0.993]	[0.539]
Placebo mean	85.7	342	61.2	278	24.6	63.0
# clusters	411	408	410	408	411	408
# obs	12,003	8,528	11,936	8,447	11,936	8,437

Notes: All variables are at the individual level, with one observation for each household member aged 6-20. All currency values are measured in constant 2018 USD PPP. Education expenditure is the total expenditure on school-related fees and non-fee expenses during the current and preceding school years for each child in the relevant age group. Non-fee expenditure includes school related supplies (e.g. books) and uniforms. The number of clusters varies across columns because some small villages have no sampled households with members aged 6-13 or 14-20. The sample size is higher for total expenditure than either of fee or non-fee expenditure. If only one type of expenditure is missing for a household, we calculate that household's total education expenditure as the non-missing component times the sample mean ratio of total education expenditure over the non-missing expenditure component. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent, age, gender and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely each of the education measures for each age group, are shown in brackets.

Table A.3: Treatment Effects on Inputs to and Outputs from Farm and Non-farm Activities

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue	Input Expenditure	Hired Labour	Technology Adoption	Labour Supply (Days)	Returns to Factors
Panel A: Farm (Agriculture and Livestock)						
Asp&Plan	16.2	9.8	-0.6	0.088	15.7	7.0
	(47.6)	(9.3)	(15.1)	(0.103)	(9.8)	(18.2)
	[.]	[1.000]	[1.000]	[.]	[.]	
Cash	63.1	83.3***	27.8	0.209**	19.9*	-17.8
	(48.3)	(10.3)	(19.4)	(0.095)	(10.7)	(19.3)
	[.]	[0.001]	[0.083]	[.]	[.]	
Combined	20.0	93.4***	63.3***	0.152	2.9	-16.8
	(44.9)	(10.5)	(21.3)	(0.107)	(10.7)	(20.0)
	[.]	[0.001]	[0.002]	[.]	[.]	
P: cash	0.355	0.000	0.115	0.206	0.663	0.144
= asp&plan	[.]	[0.001]	[0.062]	[.]	[.]	
P: cash	0.363	0.429	0.127	0.526	0.113	0.959
= combined	[.]	[0.341]	[0.341]	[.]	[.]	
P: asp&plan	0.935	0.000	0.002	0.549	0.193	0.199
= combined	[.]	[0.001]	[0.002]	[.]	[.]	
P: cash	0.378	0.985	0.202	0.288	0.025	0.822
+ asp&plan	[.]	[0.971]	[0.676]	[.]	[.]	
= combined						
Placebo mean	733	163	99.7	2.90	341	146
# clusters	413	413	413	413	413	413
# obs	7,242	7,242	7,243	7,235	7,240	7,232
	Revenue	Input Expenditure	Hired Labour	Technology Adoption	Labour Supply (Days)	Profits
Panel B: Non-farm						
Asp&Plan	284**	174**	16.8	0.084**	9.0*	109
	(136)	(87)	(14.3)	(0.035)	(5.3)	(70)
	[.]	[0.100]	[0.138]	[.]	[.]	
Cash	452***	305***	15.2	0.082**	18.2***	213***
	(139)	(92)	(16.9)	(0.036)	(6.0)	(72)
	[.]	[0.002]	[0.226]	[.]	[.]	
Combined	557**	442**	17.1	0.116***	13.4**	167
	(275)	(187)	(17.2)	(0.042)	(6.6)	(114)
	[.]	[0.039]	[0.190]	[.]	[.]	
P: cash	0.269	0.166	0.915	0.954	0.095	0.167
= asp&plan	[.]	[0.498]	[0.843]	[.]	[.]	
P: cash	0.723	0.481	0.908	0.388	0.479	0.706
= combined	[.]	[1.000]	[1.000]	[.]	[.]	
P: asp&plan	0.191	0.063	0.979	0.421	0.453	0.544
= combined	[.]	[0.144]	[0.958]	[.]	[.]	
P: cash	0.500	0.832	0.495	0.339	0.100	0.209
+ asp&plan	[.]	[1.000]	[1.000]	[.]	[.]	
= combined						
Placebo mean	815	478	43.2	0.350	106	313
# clusters	413	413	413	413	413	413
# obs	7,241	7,082	7,243	7,243	7,240	7,031

Notes: All variables are at the household level and scaled to annual figures. All currency values are measured in constant 2018 USD PPP. Farm activities capture agricultural and livestock related production. Non-farm activities capture production in non-farm enterprises owned or operated by household members. Revenue, input expenditure, hired labour expenditure and labour supply are measured as in Table 1. Technology adoption for farm activities is a dummy equal to one if the household used one of 14 modern agricultural practices since intervention. For non-farm enterprises, it is one if during the last 12 months, an enterprise introduced new or improved products or services or went into a new market or accessed new customers. For farm enterprises, returns to factors of production is revenue minus expenditure on intermediate inputs and costs of renting in assets minus costs of hired labour. For non-farm enterprises, profits are revenue minus input expenditure and hired labour. Model specification is as in Table 2. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within the family 'Inputs and Hired Labour' are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.4: Treatment Effects of Total Labour Supply by Household Member Type

	(1)	(2)	(3)	(4)
	Adults	Respondent	Non-respondent Adults	Non-adults
Asp&Plan	26.8**	14.0*	13.0	-0.29
	(11.6)	(7.1)	(8.7)	(6.11)
Cash	27.2**	14.4*	12.4	1.20
	(12.4)	(7.8)	(8.1)	(5.65)
Combined	9.0	10.0	-2.3	-8.15
	(11.5)	(7.5)	(8.4)	(5.78)
P: cash = asp&plan	0.972	0.956	0.943	0.807
P: cash = combined	0.127	0.564	0.071	0.100
P: asp&plan = combined	0.118	0.574	0.100	0.206
P: cash + asp&plan = combined	0.007	0.078	0.023	0.277
Placebo mean	525	315	216	76.9
# clusters	413	413	413	413
# obs	7,240	7,121	7,240	7,240

Notes: All variables are at the household level, measured in days and scaled to annual figures. Aggregate labour supply is defined as the total days of labour supplied to: a) household farm activities which consist of agricultural and livestock activities, b) household non-farm enterprises and c) casual and salaried employment outside the household. The aggregate labour supply of the household can be subdivided into different categories. Column (1) reports the aggregate labour supply of all household members who are adults, defined as being aged 16 and above. Columns (2), (3) and (4) report the aggregate labour supply of the respondent, other non-respondent adults in the household and non-adult members of the household respectively. Adults are defined as household members aged $\geq$ 16. For the endlined sample, the mean numbers of adult members (including the respondent) is 2.8 and the mean number of non-adult members is 2.8. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.5: Treatment Effects on Labour Supply and Earnings

	(1)	(2)	(3)	(4)	(5)
	Labour Supply				Earnings
	Total	Farm	Non-farm	Casual and Salaried	Casual and Salaried
Asp&Plan	26.8**	15.7	9.0*	2.78	-24.5
	(11.6)	(9.8)	(5.3)	(4.70)	(33.2)
	[.]	[0.201]	[0.201]	[0.227]	[.]
Cash	27.2**	19.9*	18.2***	-11.00**	-71.2**
	(12.4)	(10.7)	(6.0)	(4.78)	(35.0)
	[.]	[0.035]	[0.008]	[0.023]	[.]
Combined	9.0	2.9	13.4**	-7.53*	-55.9*
	(11.5)	(10.7)	(6.6)	(4.37)	(32.0)
	[.]	[0.357]	[0.149]	[0.149]	[.]
P: cash = asp&plan	0.972	0.663	0.095	0.007	0.182
	[.]	[0.284]	[0.106]	[0.022]	[.]
P: cash = combined	0.127	0.113	0.479	0.481	0.654
	[.]	[0.511]	[0.511]	[0.511]	[.]
P: asp&plan = combined	0.118	0.193	0.453	0.036	0.332
	[.]	[0.240]	[0.409]	[0.121]	[.]
P: cash + asp&plan	0.007	0.025	0.100	0.921	0.407
= combined	[.]	[0.082]	[0.111]	[0.443]	[.]
Placebo mean	525	341	106	78.8	451
# clusters	413	413	413	413	413
# obs	7,240	7,240	7,240	7,240	7,240

Notes: All variables are at the household level and scaled to annual figures. Labour supply variables are measured in days while the earnings variable is in constant 2018 USD PPP. All variables refer to labour supplied by adult members of the household. Aggregate labour supply is defined as the total days of labour supplied to: a) household farm activities which consist of agricultural and livestock activities, b) household non-farm enterprises and c) casual and salaried employment outside the household. Earnings from casual and salaried employment include the total monetary value of earnings in cash and in-kind from farming another household's land, tending animals owned by other households, any other casual work and from salaried employment working for someone outside the household. Adults are defined as household members aged >=16. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.6: Treatment Effects on Assets

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Non-land Asset Components					
	Non-land Assets	Durables	Livestock	Savings	Maize	Land Purchases	Housing Expenditures
Asp&Plan	98**	43*	25.4	26.4**	2.97	12.8	18
	(46)	(25)	(26.9)	(11.7)	(4.70)	(8.0)	(26)
	[.]	[0.144]	[0.299]	[0.113]	[0.359]	[.]	[.]
Cash	406***	236***	132.4***	29.8***	9.35*	34.6**	487***
	(50)	(31)	(27.7)	(10.1)	(5.26)	(14.0)	(34)
	[.]	[0.001]	[0.001]	[0.003]	[0.020]	[.]	[.]
Combined	352***	224***	77.0***	46.2***	7.92	43.8***	465***
	(47)	(27)	(26.5)	(16.2)	(5.51)	(10.6)	(33)
	[.]	[0.001]	[0.005]	[0.005]	[0.040]	[.]	[.]
P: cash = asp&plan	0.000	0.000	0.000	0.787	0.267	0.062	0.000
	[.]	[0.001]	[0.001]	[0.553]	[0.217]	[.]	[.]
P: cash = combined	0.292	0.689	0.053	0.341	0.810	0.566	0.591
	[.]	[1.000]	[0.269]	[1.000]	[1.000]	[.]	[.]
P: asp&plan = combined	0.000	0.000	0.067	0.204	0.390	0.006	0.000
	[.]	[0.001]	[0.112]	[0.158]	[0.243]	[.]	[.]
P: cash + asp&plan	0.025	0.167	0.040	0.597	0.552	0.854	0.430
= combined	[.]	[0.335]	[0.192]	[0.502]	[0.502]	[.]	[.]
Placebo mean	1,529	765	576	122	65.6	9.22	196
# clusters	413	413	413	413	412	413	413
# obs	7,242	7,242	7,243	7,241	7,170	7,239	7,168

Notes: All variables are at the household level and scaled to annual figures. All currency values are measured in constant 2018 USD PPP. Non-land assets are made up of durable assets, livestock, savings and stocks of dried maize. Respondents estimate the value of household holdings of each asset if they were to sell them today in their current condition. For cash savings, we include savings in multiple places, as well as the payout households receive from ROSCAs to which they belong. Land purchases are the value of any compound and non-compound land that was purchased since the intervention. Housing expenditures includes the costs of any housing repairs, maintenance and construction since intervention. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within the family of non-land non-housing assets are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.7: Treatment Effects on Aspirations

	(1)	(2)	(3)	(4)
		Index Components		
	Aspirations Index	Assets	Income	Education
Asp&Plan	0.092***	265	127	0.226***
	(0.035)	(565)	(216)	(0.067)
	[.]	[0.742]	[0.742]	[0.003]
Cash	0.130***	2,008***	611***	0.057
	(0.036)	(624)	(205)	(0.076)
	[.]	[0.005]	[0.005]	[0.178]
Combined	0.178***	2,511***	570**	0.130*
	(0.040)	(606)	(242)	(0.072)
	[.]	[0.001]	[0.020]	[0.030]
P: cash = asp&plan	0.324	0.007	0.030	0.019
	[.]	[0.022]	[0.022]	[0.022]
P: cash = combined	0.258	0.457	0.874	0.292
	[.]	[1.000]	[1.000]	[1.000]
P: asp&plan = combined	0.027	0.000	0.062	0.138
	[.]	[0.002]	[0.066]	[0.101]
P: cash + asp&plan	0.398	0.788	0.602	0.105
= combined	[.]	[1.000]	[1.000]	[0.463]
Placebo mean	0.000	8,499	5,357	15.5
# clusters	413	413	413	410
# obs	7,232	7,204	7,185	6,102

Notes: Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). The index definition includes all observations that have non-missing values for at least one component, using only the non-missing components in the averaging. Income aspirations are the level of annual income that a household would like to reach at the end of the next ten years. Income is defined as all sources of cash income for the household, including earnings from production and transfers from any NGO or government programmes. Asset aspirations are the level of assets that the household would like to reach at the end of the next ten years, including their house, furniture, consumer goods and a transport vehicles. Income and assets are measured in constant 2018 USD PPP. Education aspirations are the aspirations for years of education attained by a randomly selected child, set to missing for households without children. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.8: Additional Treatment Effects on Psychological Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
		Index Components				
	Self-beliefs Index	Self-Efficacy	Growth Mindset	Locus of Control	Depression Score >10	Depression Score >13
Asp&Plan	0.006	-0.138	0.305	-0.049	0.010	0.004
	(0.046)	(0.143)	(0.224)	(0.096)	(0.018)	(0.019)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Cash	-0.053	-0.206	-0.032	-0.110	-0.031**	-0.038**
	(0.042)	(0.138)	(0.214)	(0.094)	(0.016)	(0.019)
	[.]	[0.568]	[0.568]	[0.568]	[0.050]	[0.050]
Combined	0.025	0.033	0.119	0.048	-0.030*	-0.024
	(0.044)	(0.145)	(0.206)	(0.097)	(0.016)	(0.018)
	[.]	[1.000]	[1.000]	[1.000]	[0.120]	[0.120]
P: cash = asp&plan	0.198	0.628	0.131	0.552	0.021	0.023
	[.]	[0.720]	[0.646]	[0.720]	[0.024]	[0.024]
P: cash = combined	0.043	0.057	0.420	0.097	0.933	0.355
	[.]	[0.170]	[0.170]	[0.170]	[1.000]	[1.000]
P: asp&plan = combined	0.687	0.221	0.391	0.355	0.020	0.110
	[.]	[0.642]	[0.642]	[0.642]	[0.043]	[0.059]
P: cash + asp&plan	0.224	0.041	0.601	0.125	0.701	0.659
= combined	[.]	[0.720]	[0.646]	[0.720]	[0.024]	[0.024]
Placebo mean	0.000	24.1	23.1	17.4	0.658	0.462
# clusters	413	413	413	413	413	413
# obs	7,221	7,211	7,209	7,213	7,213	7,213

Notes: Column (1) is an Anderson (2008) index consisting of variables in columns (2), (3) and (4). The index definition includes all observations that have non-missing values for at least one component, using only the non-missing components in the averaging. The self-beliefs index is made up of growth mindset, self-efficacy, and locus of control scales. Self-efficacy is measured with the Schwarzer and Jerusalem (1995) scale. Growth mindset is measured with an adapted version of the 6-item Implicit Theories of Intelligence scale (Blackwell et al., 2007). Locus of control is measured using the Internal subscale from the Internal, Powerful Others and Chance (IPC) scale (Levenson, 1981). In Table 3, mental health is the 10-item CES-D depression scale from Andresen et al. (1994), multiplied by minus one. Here, we generate binary variables where individuals with scores at or above a threshold are identified as at high risk of depression or as experiencing psychological distress. This is how the score is used if it is used for clinical screening. Different studies in sub-Saharan Africa use thresholds of 13 (Baron et al., 2017) or 10 (Kilburn et al., 2016). Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family, namely self-beliefs and depression, are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.9: Treatment Effects on Beliefs About Returns to Specific Investments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Maize Farming					Education		
	Yields			Returns		Income		Returns
	Usual	w/ Fertiliser	w/ Labour	Fertiliser	Labour	Secondary	Degree	Degree
Asp&Plan	2.95	-6.65	-4.34	0.009	-0.036	-193.8	-392	0.041
	(6.91)	(12.57)	(9.81)	(0.077)	(0.031)	(140.9)	(341)	(0.139)
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
Cash	3.50	15.40	-8.75	0.152*	-0.064**	-78.5	-200	0.126
	(7.15)	(13.85)	(9.41)	(0.078)	(0.030)	(138.8)	(315)	(0.146)
	[.]	[.]	[.]	[0.086]	[0.086]	[.]	[.]	[0.149]
Combined	1.59	-4.16	-10.96	0.025	-0.034	-67.9	-98	0.053
	(6.99)	(12.75)	(10.08)	(0.074)	(0.034)	(125.2)	(330)	(0.145)
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
P: cash = asp&plan	0.947	0.130	0.648	0.100	0.364	0.461	0.570	0.550
	[.]	[.]	[.]	[0.430]	[0.574]	[.]	[.]	[0.579]
P: cash = combined	0.798	0.159	0.808	0.093	0.338	0.933	0.745	0.581
	[.]	[.]	[.]	[0.387]	[0.510]	[.]	[.]	[0.633]
P: asp&plan = combined	0.859	0.849	0.522	0.835	0.950	0.342	0.381	0.931
	[.]	[.]	[.]	[1.000]	[1.000]	[.]	[.]	[1.000]
P: cash + asp&plan	0.617	0.483	0.874	0.190	0.118	0.255	0.273	0.542
= combined	[.]	[.]	[.]	[0.398]	[0.398]	[.]	[.]	[0.398]
Placebo mean	191	459	258	2.02	0.442	3,684	11,695	3.32
# clusters	413	411	410	411	410	410	410	410
# obs	7,115	6,420	6,147	6,420	6,147	5,137	5,137	5,137

Notes: Columns (1), (2), and (3) show the expected yield of maize with respectively no fertiliser and their current labour input, 50kg of fertiliser per acre and their current labour input, and no fertiliser and 12 hours of extra labour per week. The yields are for a one acre plot in the next long rains season, measured in gorogoro (a local unit approximately equal to 2 kilograms) per acre. The questions ask about DAP fertiliser, the most commonly used fertiliser in the region. Columns (4) and (5) show the implied returns from respectively extra fertiliser and extra labour, with a scale where 1 = 100% return. Columns (6) and (7) show the expected annual income for the respondent's child in two scenarios: complete secondary schooling with a KCSE certificate and a university degree. Column (8) shows the implied return to a university degree, with a scale where 1 = 100% return. The questions ask about the same child as the education aspirations and expectations questions, at age 30, with earnings expressed in 2018 USD PPP. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across the three returns measures are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.10: Tests for Social Desirability Bias, Mimicry and Differential Information Recall

	(1)	(2)	(3)	(4)	(5)	(6)
	Housing quality		Asset ownership		Recall & Mimicry	
	Self-reported	Verified	Self-reported	Verified	Information Recall	Video Mimicry
Asp&Plan	18	0.009	-0.027	0.06	-0.040	0.005
	(26)	(0.037)	(0.352)	(0.41)	(0.025)	(0.042)
Cash	487***	0.515***	1.758***	1.58***	-0.016	-0.022
	(34)	(0.042)	(0.361)	(0.40)	(0.026)	(0.039)
Combined	465***	0.531***	0.933***	1.17***	-0.024	0.052
	(33)	(0.041)	(0.318)	(0.40)	(0.024)	(0.045)
Placebo mean	196	1.56	20.6	17.3	0.274	-0.000
# clusters	413	413	413	413	409	413
# obs	7,168	7,242	7,243	7,243	6,536	7,235

Self-reported housing expenditure is household expenditure on repair, maintenance or construction of housing since intervention. For the verified housing quality score, fieldworkers rated materials used to construct the roof, wall and floor based on expense. Roof materials ratings are: 0 = leaves, grass or tins; 1=iron, cement, tiles or asbestos. Walls material ratings are: 0 = mud, or unburnt bricks; 1 = iron/tin sheets, wood, or mud and cement; 2 = burnt/stabilised brick, cement blocks, or concrete and stones. Floor materials scores are: 0 = mud/earth, other organic, or part organic, part finished; 1 = wood, cement or tiles. Field officer-verified assets are fieldworker observations of the number of seven durable assets cooking pots and pans, jerry cans, chairs/sofa, tables, radios, TVs and poultry houses, counted after the survey finished. The self-reported assets are the number of those specific assets reported in the assets module earlier in the survey. Information recall is an indicator equal to one if respondents correctly recall information about the returns to education for Kenyan men from Ozier (2018), which appears in both the aspirations and planning and placebo video. This question is asked the same day that respondents watch the videos. Mimicry of videos is the standardised sum of indicator variables coded to one if the respondent engaged in any of the following activities at endline, all of which are featured in the videos: (a) weaved baskets; (b) kept savings in a jar; (c) attended a sewing class; (d) trained as a teacher; (e) grew vegetables to sell on the market.

Table A.11: Tests for Social Desirability Bias Using Self-Reported and Verified Measures of Asset Ownership

	(1) Asset Ownership	(2) Cooking Pots/Pans	(3) Jerry Cans	(4) Chairs / Sofas	(5) Tables / Desks	(6) Radios	(7) TVs / CD Players	(8) Livestock Houses
Panel A: Self-reported								
Asp&Plan	-0.027	0.041	-0.142	0.008	0.047	-0.035	0.024	0.030*
	(0.352)	(0.112)	(0.210)	(0.124)	(0.032)	(0.021)	(0.018)	(0.016)
Cash	1.758***	0.386***	0.200	0.819***	0.201***	0.058***	0.095***	0.005
	(0.361)	(0.114)	(0.199)	(0.144)	(0.030)	(0.021)	(0.018)	(0.015)
Combined	0.933***	0.289**	-0.159	0.529***	0.122***	0.066***	0.066***	0.023
	(0.318)	(0.116)	(0.195)	(0.150)	(0.030)	(0.020)	(0.018)	(0.020)
P: cash = asp&plan	0.000	0.002	0.069	0.000	0.000	0.000	0.000	0.132
P: cash = combined	0.012	0.369	0.048	0.059	0.008	0.639	0.108	0.345
P: asp&plan = combined	0.004	0.031	0.923	0.000	0.023	0.000	0.028	0.753
P: cash + asp&plan = combined	0.106	0.381	0.436	0.124	0.005	0.134	0.040	0.630
Placebo mean	20.6	5.81	6.75	5.57	1.56	0.676	0.170	0.129
# clusters	413	413	413	413	413	413	413	413
# obs	7,243	7,239	7,238	7,239	7,238	7,239	7,238	7,237
Panel B: Field Officer-Verified								
Asp&Plan	0.06	0.086	-0.138	0.029	0.047	-0.013	0.027*	0.022*
	(0.41)	(0.148)	(0.209)	(0.141)	(0.041)	(0.026)	(0.015)	(0.013)
Cash	1.58***	0.275*	0.164	0.789***	0.205***	0.071**	0.071***	0.017
	(0.40)	(0.153)	(0.197)	(0.151)	(0.040)	(0.028)	(0.014)	(0.011)
Combined	1.17***	0.235	-0.055	0.596***	0.189***	0.132**	0.064***	0.017
	(0.40)	(0.153)	(0.182)	(0.181)	(0.064)	(0.054)	(0.017)	(0.013)
P: cash = asp&plan	0.000	0.193	0.109	0.000	0.000	0.002	0.008	0.729
P: cash = combined	0.322	0.780	0.193	0.315	0.807	0.269	0.693	0.970
P: asp&plan = combined	0.008	0.334	0.650	0.001	0.016	0.002	0.033	0.722
P: cash + asp&plan = combined	0.427	0.553	0.770	0.342	0.380	0.184	0.126	0.195
Placebo mean	17.3	4.52	5.52	5.11	1.39	0.542	0.129	0.087
# clusters	413	413	413	413	413	413	413	413
# obs	7,243	7,239	7,238	7,239	7,238	7,239	7,238	7,237

Notes: Field officer verified assets are objective measures of the number of seven durable assets – cooking pots and pans, jerry cans, chairs/sofa, tables, radios, TVs and poultry houses. The field officer was asked to count these assets after completing the endline survey. The self-reported assets are the number of those specific assets reported in the assets module earlier in the survey. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.12: Spillover Effects of the Aspirations and Planning Workshop on Economic Outcomes of Ineligible Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Expenditure	Revenue	Non-Land Assets	Consumption Expenditure
Asp&Plan	-0.007	-8.20	62	-3.49	210	-31	-25.1
	(0.049)	(27.44)	(140)	(64.51)	(258)	(108)	(123.0)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	
Placebo mean	0.000	549	1,053	851	2,573	2,264	3,839
# clusters	306	306	306	303	306	306	306
# obs	2,792	2,789	2,792	2,154	2,792	2,792	2,783

Notes: This table shows treatment effects of the aspirations and planning workshop on economic outcomes for ineligible households living in the study villages. This sample was only surveyed in a subset of study villages. Each of the economic variables is defined in Table 1. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.13: Spillover Effects of Aspirations and Planning Workshop on Psychological Mechanisms of Ineligible Households

	(1)	(2)	(3)	(4)
	Self-belief Index	Aspirations Index	Expectations Index	Mental health Z-score
Asp&Plan	0.047	-0.010	-0.010	0.099*
	(0.068)	(0.051)	(0.065)	(0.055)
	[1.000]	[1.000]	[1.000]	[0.390]
Placebo mean	0.000	0.000	-0.000	-0.000
# clusters	306	306	306	306
# obs	2,785	2,784	2,782	2,776

Notes: This table shows treatment effects of the aspirations and planning workshop on psychological outcomes for ineligible households living in the study villages. Each of the psychological variables are defined in Section 3.1. We did not measure time or risk preferences or beliefs about returns to investments in this sample. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. For each outcome variable, we report the coefficients of interest and heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.14: Treatment Effects of the Aspirations and Planning Workshop at Different Levels of Baseline Economic Resources

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Index Components					
	Economic Index	Labour Supplied (Days)	Inputs & Hired Labour	Education Expenditure	Revenue	Non-Land Assets	Consumption Expenditure
Panel A: Consumption as wealth proxy							
Asp&Plan	0.112***	27.1**	230**	268*	23.0	99.8**	143*
	(0.035)	(11.7)	(99)	(154)	(27.1)	(45.8)	(74)
	[.]	[0.064]	[0.064]	[0.064]	[0.109]	[0.064]	[0.064]
Interaction	0.000	-0.002	0.088*	0.062	-0.003	0.035	-0.027
* wealth	(0.000)	(0.005)	(0.047)	(0.069)	(0.011)	(0.021)	(0.031)
	[.]	[1.000]	[0.439]	[0.600]	[1.000]	[0.439]	[0.600]
Placebo mean	-0.000	525	857	2,101	640	1,529	3,796
# clusters	413	413	413	413	412	413	412
# obs	7,243	7,240	7,243	7,243	6,273	7,242	7,224
Panel B: Non-land assets as wealth proxy							
Asp&Plan	0.110***	26.2**	232**	257*	22.7	96.1**	139*
	(0.035)	(11.6)	(102)	(156)	(26.8)	(45.6)	(75)
	[.]	[0.077]	[0.077]	[0.077]	[0.136]	[0.077]	[0.077]
Interaction	0.000	-0.006	0.109	-0.059	-0.024	0.031	0.033
* wealth	(0.000)	(0.008)	(0.080)	(0.112)	(0.020)	(0.056)	(0.058)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Placebo mean	-0.000	525	857	2,101	640	1,529	3,796
# clusters	413	413	413	413	412	413	412
# obs	7,243	7,240	7,243	7,243	6,273	7,242	7,224
Panel C: Assets including land as wealth proxy							
Asp&Plan	0.111***	26.8**	229**	261*	21.2	96.0**	139*
	(0.036)	(11.6)	(101)	(156)	(27.3)	(45.8)	(75)
	[.]	[0.076]	[0.076]	[0.079]	[0.130]	[0.076]	[0.076]
Interaction	-0.000	-0.001	0.006	0.001	-0.001	-0.001	0.006
* wealth	(0.000)	(0.001)	(0.006)	(0.010)	(0.003)	(0.005)	(0.005)
	[.]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Placebo mean	-0.000	525	857	2,101	640	1,529	3,796
# clusters	413	413	413	413	412	413	412
# obs	7,243	7,240	7,243	7,243	6,273	7,242	7,224

Notes: This table shows heterogeneous treatment effects of the aspirations and planning workshop on economic outcomes. Coefficients are from an OLS regression of each outcome on a vector of treatment assignments, a wealth proxy, the interaction of treatment assignments and the wealth proxy, the baseline outcome, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. Each panel shows results from using a separate wealth proxy. The wealth proxy in panel C is the value of all non-loan assets including the respondents' assessment of the value of their land and housing. All other proxies and all outcomes are defined in Table 1. We report heteroskedasticity-robust standard errors, clustered at village level, in parentheses. Sharpened q-values controlling for the false discovery rate across outcomes within each family are shown in brackets. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Table A.15: Treatment Effects on the Investment Share of Total Expenditure

	(1)	(2)	(3)
	Inv. share	Inv. share	Inv. share
Cash	0.0307***	0.0175***	0.0176**
	(0.0061)	(0.0053)	(0.0053)
Combined	0.0395***	0.0256***	0.0254***
	(0.0068)	(0.0054)	(0.0054)
Total expenditure (1000s)		0.0198***	0.0211***
		(0.0007)	(0.0010)
Cash X total expenditure (1000s)			-0.0020
			(0.0014)
Combined X total expenditure (1000s)			-0.0014
			(0.0015)
Placebo mean outcome	0.192	0.192	0.192
Placebo mean regressor		5.388	5.388
Placebo std dev. regressor		3.139	3.139
# clusters	412.000	412.000	412.000
# obs	7156.000	7116.000	7116.000

Notes: This table shows treatment effects of the cash and combined interventions on the share of total expenditure that is invested. Coefficients in column (1) are from an OLS regression on a vector of treatment assignments, sublocation fixed effects, endline month fixed effects, an indicator for the endline being answered by a proxy respondent and prespecified baseline covariates. Coefficients in column (2) are from a regression that adds total expenditure and coefficients in column (3) are from a regression that also adds interactions between treatment assignments and total expenditure. All expenditure and investment share measures have the top percentile trimmed to reduce sensitivity to outliers. Total expenditure is reported in 1000s of USD PPP and is the sum of expenditure on consumption, education, productive inputs, and hired labour; total investment excludes expenditure on consumption. We report heteroskedasticity-robust standard errors, clustered at village level, in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

A.2 Associations Between Aspirations, Expectations and Other Measures

Table A.16: Pairwise Correlations of Psychological Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AI	DF	NPB	RTI	S-bI	RI	MHZ	EI
Aspirations Index	1.00	-0.05	0.02	0.02	0.07	0.04	0.10	0.60
Discount Factor	-0.05	1.00	0.26	-0.00	-0.02	-0.02	-0.04	-0.05
No Present bias	0.02	0.26	1.00	0.01	-0.01	0.01	-0.00	0.01
Risk Taking Index	0.02	-0.00	0.01	1.00	0.02	0.03	-0.00	0.03
Self-belief Index	0.07	-0.02	-0.01	0.02	1.00	-0.07	0.09	0.04
Returns Index	0.04	-0.02	0.01	0.03	-0.07	1.00	0.02	0.01
Mental Health Z-score	0.10	-0.04	-0.00	-0.00	0.09	0.02	1.00	0.14
Expectations Index	0.60	-0.05	0.01	0.03	0.04	0.01	0.14	1.00

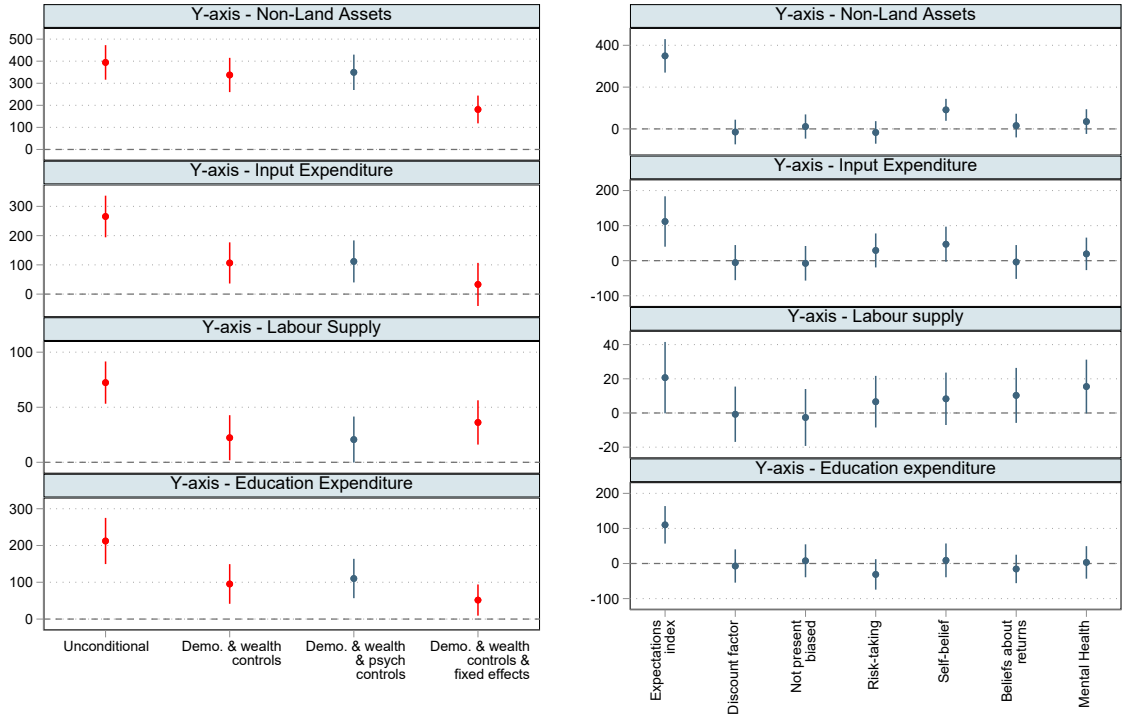
Notes: This table shows pairwise correlations between psychological outcomes, all defined in Section 3.1.

Table A.17: Relationship Between Aspirations, Expectations, and Other Psychological Outcomes

	(1)	(2)	(3)	(4)
	Asp index	Asp index	Exp index	Exp index
	OLS	LASSO	OLS	LASSO
Discount factor	-0.125		-0.121	
	(0.12)		(0.12)	
No present bias	0.052		0.030	
	(0.34)		(0.57)	
Risk-taking	0.012		0.022	
	(0.63)		(0.36)	
Self-beliefs index	0.062***		0.017	
	(0.01)		(0.45)	
Beliefs about returns	0.036		0.007	
	(0.20)		(0.79)	
Mental health Z-score	0.086***	0.013	0.141***	0.063
	(0.00)		(0.00)	
Observations	1716	1716	1717	1717
R^2	0.02		0.02	

This table shows the relationship between the aspirations and expectations indices and other psychological measures defined in Figure 1 in the placebo group at endline. Odd columns are the estimates from OLS regressions of the Aspirations Index (1) and Expectations Index (3) on the other psychological mechanisms. Even columns are the estimates from LASSO regressions of the Aspirations Index (2) and Expectations Index (4) on the other psychological mechanisms, with the lamba parameter chosen using cross validation. Heteroskedasticity-robust standard errors shown in parentheses for the OLS estimates. No standard errors shown for the LASSO estimates because post-model-selection inference is not valid. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

Figure A.1: Relationships Between Wealth, Investment, Expectations, and Other Psychological Characteristics



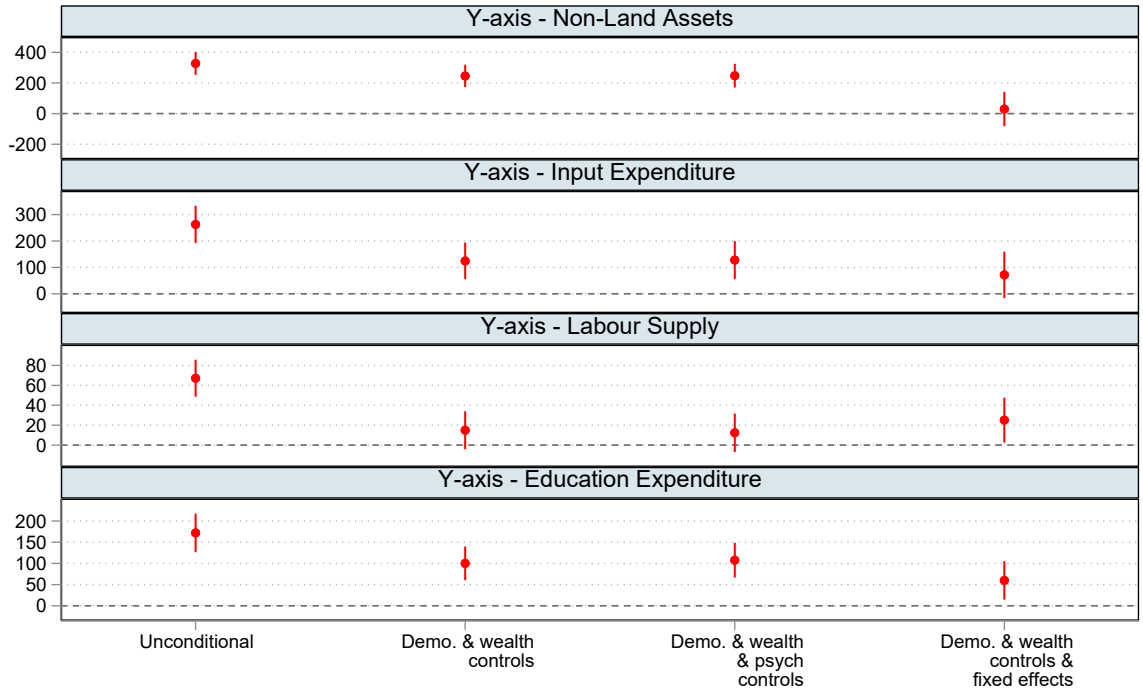
Notes: This figure shows that the wealth-aspirations and investment-aspirations relationships from Section 3.2 and Figure 1 are very similar when we replace the standardised aspirations index with a standardised expectations index.

The four vertically stacked left panels show coefficients and 95% confidence intervals from regressing a wealth proxy (non-land assets) and three investment measures (input expenditure, labour supply and education expenditure) on the standardised expectations index and other variables. Within each left-hand panel, the first column shows the coefficients on the expectations index from bivariate regressions; the second column shows the coefficients on the expectations index controlling for respondent age, education, marital status, household size, number of school-aged members, county fixed effects and (except for the top panel) asset value and consumption; the third column shows the coefficients on the expectations index controlling for the same variables and the psychological characteristics shown in the right hand panels; and the fourth column shows the coefficients on the expectations index controlling for the same variables as in the second column, plus respondent fixed effects.

The right panels show coefficients and 95% confidence intervals on all psychological variables from the same regressions that generates the third column in each left-hand panels. The right panels are identical to Figure 1 except that they replaces the aspirations index with the expectations index.

All asset and investment measures are defined in the footnote below Table 1. The expectations index and psychological measures are defined in Section 3.1. Here, each is standardised to allow for coefficient comparison. All regressions use the endline placebo group data with the top percentiles of expectations, investment, assets, and consumption trimmed. Sample size is 1376 to 1747 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.2: Relationships Between Wealth, Investment, and Aspirations Using Different Control Variables

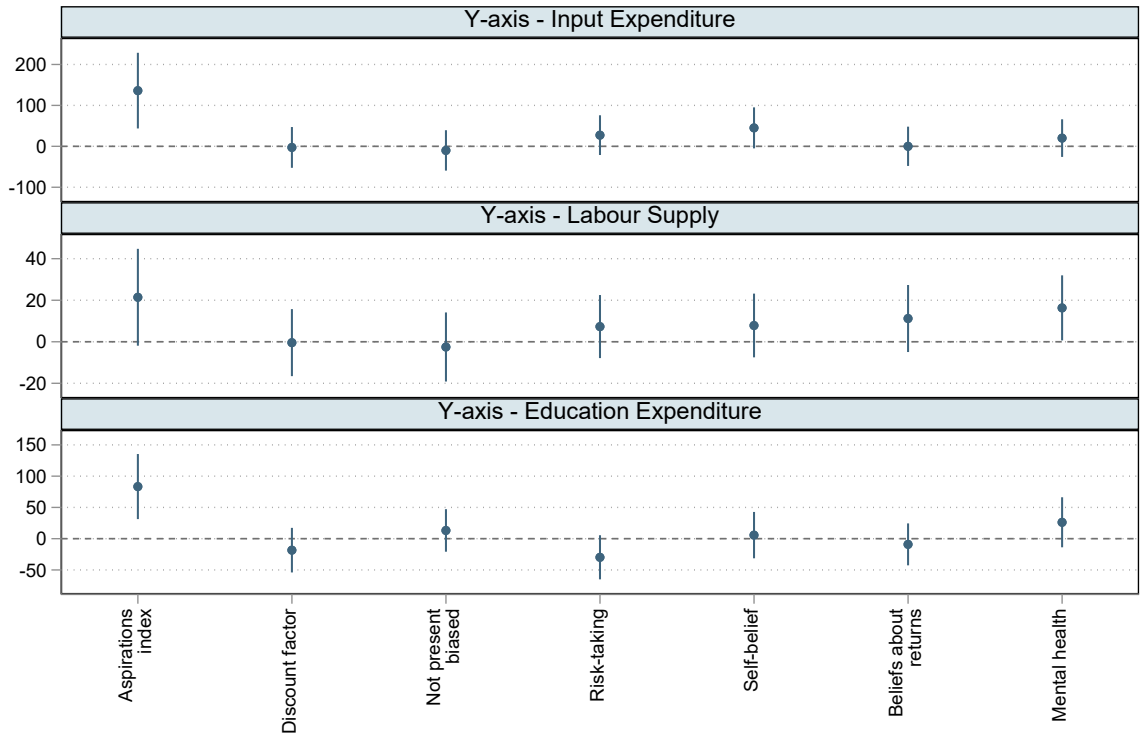


Notes: This figure shows that the wealth-aspirations and investment-aspirations relationships from Section 3.2 and Figure 1 are robust to using different control variables.

The four vertically stacked panels show coefficients and 95% confidence intervals from regressing a wealth proxy (non-land assets) and three investment measures (input expenditure, labour supply and education expenditure) on the standardised aspirations index and other variables. Within each panel, the first column shows the coefficients on the aspirations index from bivariate regressions; the second column shows the coefficients on the aspirations index controlling for respondent age, education, marital status, household size, number of school-aged members, county fixed effects and (except for the top panel) asset value and consumption; the third column shows the coefficients on the aspirations index controlling for the same variables and the psychological characteristics shown in Figure 1; and the fourth column shows the coefficients on the aspirations index controlling for the same variables as in the second column, plus respondent fixed effects.

All asset and investment measures are defined in the footnote below Table 1. The aspirations index and psychological measures are defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of expectations, investment, assets, and consumption trimmed. Sample size is 1376 to 1747 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.3: Relationships Between Investment, (Aspirations - Perceived Current Economic Position), and Other Psychological Characteristics

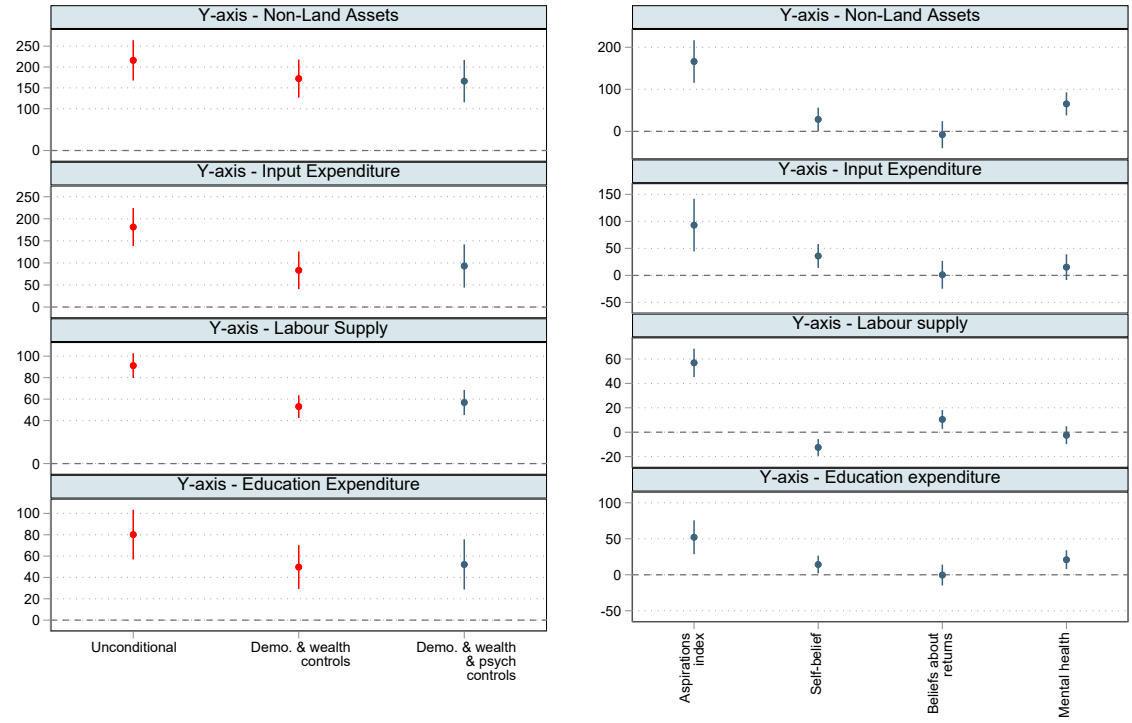


Notes: This figure shows that the investment-aspirations relationships from Section 3.2 and Figure 1 are robust to replacing the aspirations index with the aspirations index minus respondents' beliefs about their current economic position.

The three vertically stacked panels show coefficients and 95% confidence intervals from regressing three investment measures (expenditure on productive inputs and hired labour, labour supply, and education expenditure) on psychological characteristics. All regressions control for respondent age, education, marital status, household size, number of school-aged members, county fixed effects and (except for the top panel) asset value and consumption.

All asset and investment measures are defined in the footnote below Table 1. All psychological measures are defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of aspirations, investment, assets, and consumption trimmed. Sample size is 1364 to 1745 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.4: Relationships Between Wealth, Investment, and Psychological Characteristics At Baseline Using Different Control Variables

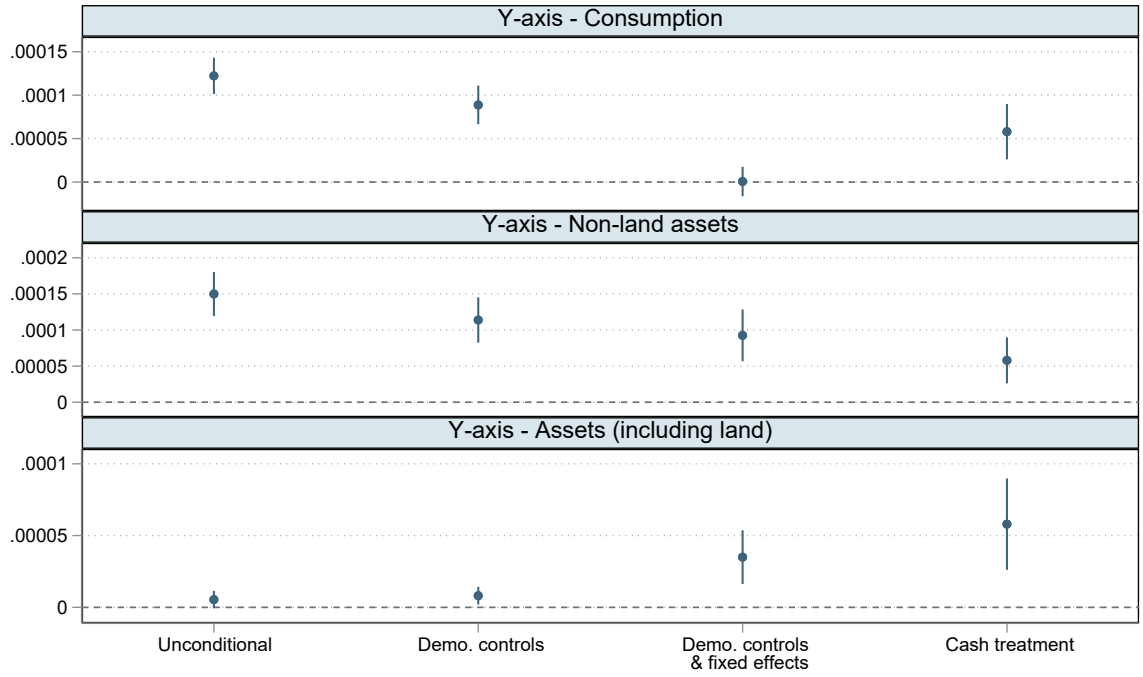


Notes: This figure shows that the wealth-aspirations and investment-aspirations relationships from Section 3.2 are very similar when we use baseline data instead of endline placebo group data. The right panels replicate Figure 1 and the left panels replicate Figure A.2. This analysis omits time and risk preferences because they were not measured at baseline.

The left panels show coefficients and 95% confidence intervals from regressing different wealth and investment measures on psychological characteristics at baseline. The top panel on the left shows results from regressing a wealth proxy (non-land assets) and the second to fourth panels show results from regressing three investment measures (input expenditure, labour supply and education expenditure). Within each left-hand panel, the first column shows the coefficients on the aspirations index from bivariate regressions; the second column shows the coefficients on the aspirations index controlling for respondent age, education, marital status, household size, number of school-aged members, county fixed effects and (except for the top panel) asset value and consumption; and the third column shows the coefficients on the aspirations index controlling for the same variables and the psychological characteristics shown in the right-hand panels. The right panels show coefficients on each of the psychological characteristics. Compared to Figures 1 and A.2, no fixed effects specification is reported as the relationship reports on cross-sectional data at baseline.

All regressions use the full baseline sample with the top percentiles of aspirations, investment, assets, and consumption trimmed. All asset and investment measures are defined in the footnote below Table 1. The psychological measures are defined in Section 3.1. Here, each is standardised to allow for coefficient comparison. Beliefs about returns is the standardised measure of beliefs about returns to fertiliser, the only belief about returns measured at baseline. Sample size is 5731 to 8175 depending on the choice of controls and investment measure. The smaller sample sizes are for education expenditure, as this is set to missing for households with no school-aged children. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.5: Relationship Between Wealth and Aspirations Using Different Control Variables

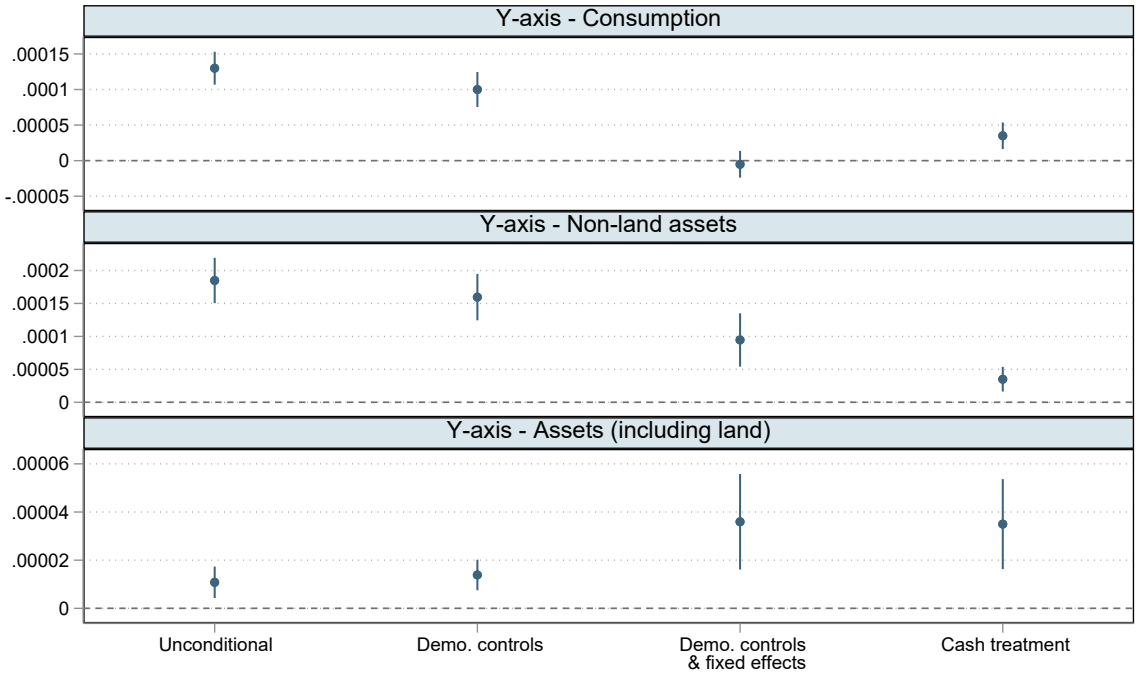


Notes: This figure shows the positive wealth-aspirations relationships discussed in Section 6.4.

The first three columns in each vertically stacked panel show the point estimates and 95% confidence intervals from regressions of aspirations on wealth, each using a different specification. The first, second, and third vertically stacked panels measure wealth using respectively the value of annual consumption, the value of non-land and non-housing assets, and the value of total assets. Within each panel, the first column is estimated using a bivariate regression of the aspirations index on each wealth proxy; the second column is from a regression including controls for respondent age, education, marital status, household size, number of school-aged members and county fixed effects; and the third column is from a regression including the same controls and respondent fixed effects using the panel data. The fourth column shows the treatment effect of the cash transfer on aspirations, divided by the 2,237 USD PPP value of the cash transfer so it has the same scale as the first three columns. This is included as a benchmark to help interpret the magnitudes of the non-experimental estimates. The estimate in the fourth column is identical in all three vertically stacked panels.

The wealth proxy measures are defined in the footnote below Table 1 and the aspirations index is defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of aspirations and wealth trimmed. Sample size is 1716 to 1743 depending on the choice of controls and wealth proxy, except for the fourth column, which uses the full endline sample. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

Figure A.6: Relationship Between Wealth and Expectations Using Different Control Variables



Notes: This figure shows the positive wealth-expectations relationships discussed in Section 6.4. It has exactly the same structure as Figure A.5 except that the aspirations index is replaced by the expectations index.

The first three columns in each vertically stacked panel show the point estimates and 95% confidence intervals from regressions of expectations on wealth, each using a different specification. The first, second, and third vertically stacked panels measure wealth using respectively the value of annual consumption, the value of non-land and non-housing assets, and the value of total assets. Within each panel, the first column is estimated using a bivariate regression of the expectations index on each wealth proxy; the second column is from a regression including controls for respondent age, education, marital status, household size, number of school-aged members and county fixed effects; and the third column is from a regression including the same controls and respondent fixed effects using the panel data. The fourth column shows the treatment effect of the cash transfer on expectations, divided by the 2,237 USD PPP value of the cash transfer so it has the same scale as the first three columns. This is included as a benchmark to help interpret the magnitudes of the non-experimental estimates. The estimate in the fourth column is identical in all three vertically stacked panels.

The wealth proxy measures are defined in the footnote below Table 1 and the expectations index is defined in Section 3.1. All regressions use the endline placebo group data with the top percentiles of expectations and wealth trimmed. Sample size is 1717 to 1743 depending on the choice of controls and wealth proxy, except for the fourth column, which uses the full endline sample. The confidence intervals are estimated using heteroskedasticity-robust standard errors.

A.3 Heterogeneous Treatment Effects

This appendix describes our tests for treatment effect heterogeneity and reports the results. We focus on three outcomes: the index of main economic outcomes (the economic index), the main mechanism outcome (the aspirations index), and one outcome in response to common questions (the mental health index). We estimate treatment effect heterogeneity across the economic index, eight prespecified baseline characteristics – age, the aspirations index, non-land asset value, education, the expectations index, household size, married (versus widowed or unmarried) and the self-beliefs index – and two non-prespecified characteristics in response to common questions – the mental health score and whether the Asp&Plan or placebo intervention was administered in a group.

We estimate heterogeneous treatment effects in two ways. First, we estimate heterogeneous treatment effects using treatment-interacted regressions:

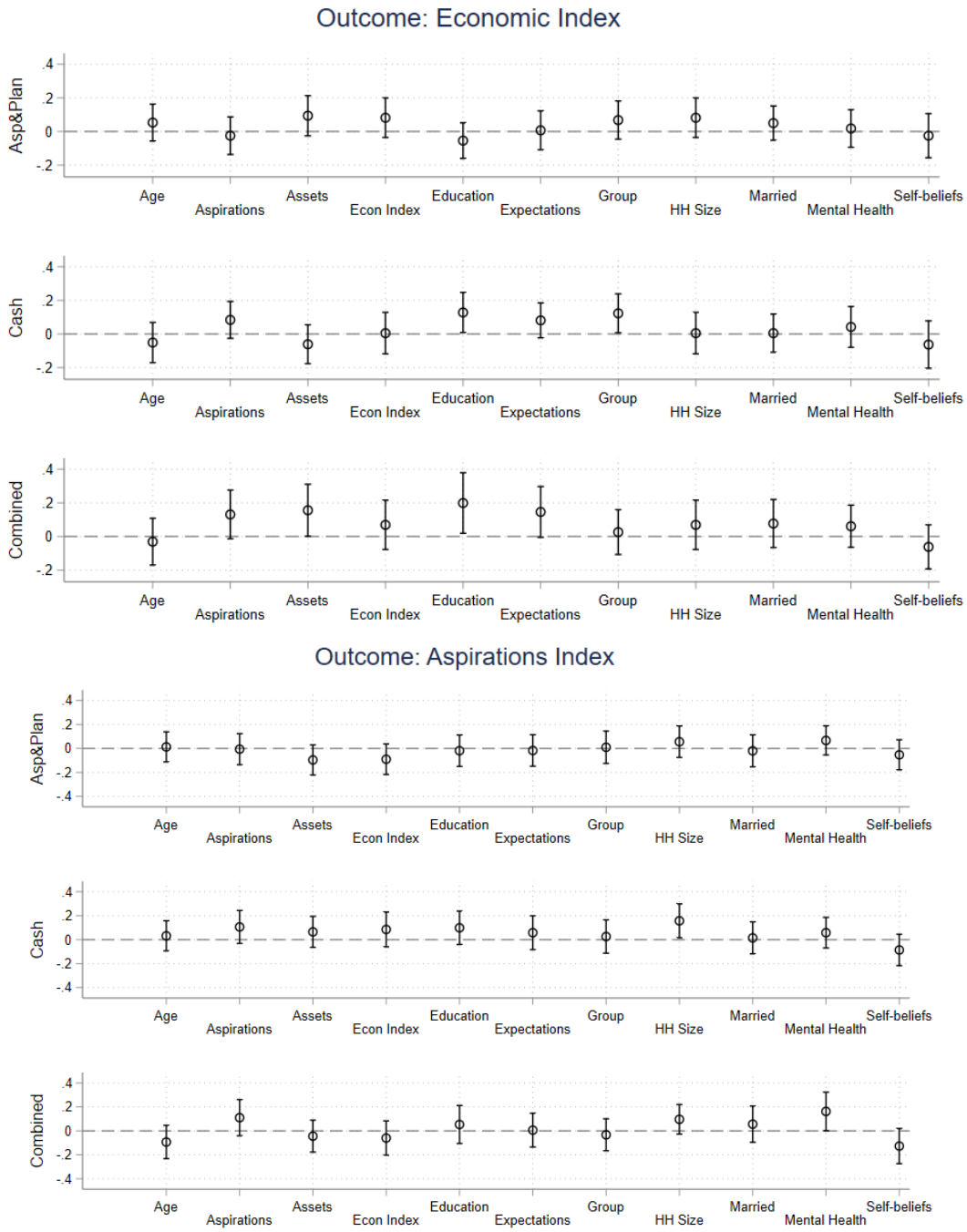
$$\begin{aligned} Y_{iv} = {} & \text{Cash}_v \cdot \beta_C + \text{Asp\&Plan}_v \cdot \beta_P + \text{Combined}_v \cdot \beta_{CP} + \mathbf{X_{iv}} \cdot \mathbf{\Gamma} \\ & + \text{Cash}_v \cdot W_{iv} \cdot \alpha_C + \text{Asp\&Plan}_v \cdot W_{iv} \cdot \alpha_P + \text{Combined}_v \cdot W_{iv} \cdot \alpha_{CP} + \epsilon_{iv} \end{aligned} \tag{5}$$

where W_{iv} is the relevant baseline characteristic. We convert all continuous measures into indicators equal to one for values above the sample median. We report the estimated interaction effects (α_C,α_P,α_B) in Figure A.7. These are seldom large and the fraction of statistically significant estimates is no larger than would arise by chance. These results provide no support for heterogeneity in treatment effects once we adjust for multiple hypothesis testing.

Second, we estimate heterogeneous effects using a causal forest (Wager and Athey, 2018). We first residualise the outcomes with respect to covariates using a standard regression forest. (Causal forests also require values of the treatment propensity score; we know these exactly from the randomization and hence do not need to estimate them.) We then run a causal forest on these residuals to generate the causal forest estimator. Estimation proceeds as follows. We randomly partition the dataset into training and testing samples in a 80/20 split. In the training dataset, we construct a set of 1001 trees, repeatedly split the data into cells based on values of the nine baseline characteristics and estimate treatment effects within these cells. Each tree is "honestly" fit: the data is used to estimate only the within-leaf treatment effect or to decide on split placement, but not both. We then generate the forest estimate by averaging these prediction rules across trees. Using separate training and testing datasets prevents overfitting. We then apply the causal forest ensemble decision rule to the testing data to estimate heterogeneous treatment effects across the cells.

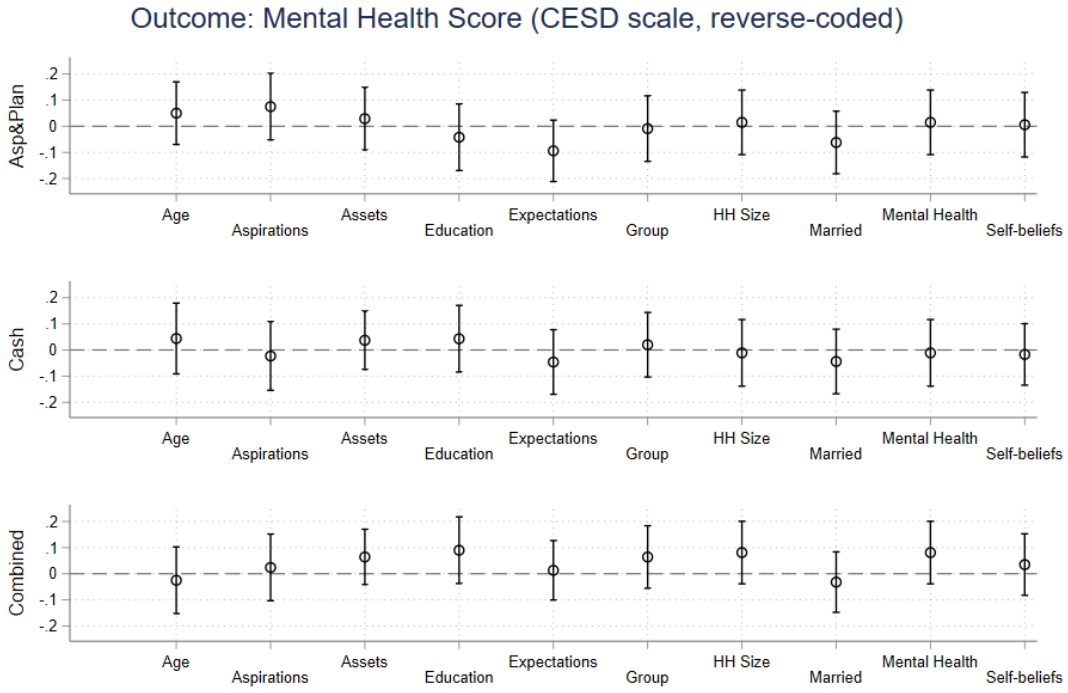
Figure A.9 shows the distribution of heterogeneous treatment effects over the cells for the workshop, cash, and combined interventions. There is limited evidence of heterogeneous treatment effects. The effects of all three treatments on the aspirations index range over the cells from

Figure A.7: Heterogeneous Treatment Effects Estimated Using Interacted Regression Models



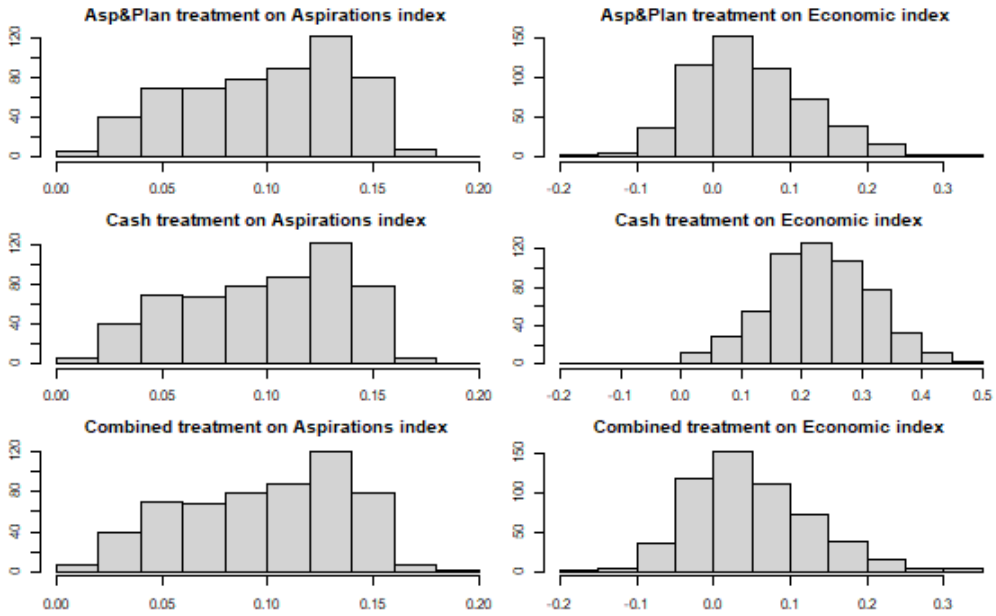
Notes: This figure shows heterogeneous treatment effects estimated using the treatment*covariate interacted regression model in equation 5. The outcome in the first panel is the summary economic index, defined in the footnote to Table 2. The outcome in the second panel is the aspirations index, defined in the footnote to Figure 1. All variables interacted with treatment are measured at baseline and continuous measures are converted into indicators equal to one for values above the sample median. All variables except the economic index, "Group", which denotes the intervention being administered in groups, and "Mental Health", which denotes the reverse-coded depression score, were prespecified. Confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by village. We estimate sharpened q-values to for the false discovery rate across dimensions of heterogeneity, within each panel. After this adjustment, no heterogeneous treatment effect is significant at the 10% significance level.

Figure A.8: Heterogeneous Treatment Effects on Mental Health Estimated Using Interacted Regression Models



Notes: This figure shows heterogeneous treatment effects estimated using the treatment*covariate interacted regression model in equation 5. The outcome is the Mental Health index, defined in the footnote to Figure 1. All variables interacted with treatment are measured at baseline and continuous measures are converted into indicators equal to one for values above the sample median. All variables except the economic index, "Group", which denotes the intervention being administered in groups, and "Mental Health", which denotes the reverse-coded depression score, were prespecified. Confidence intervals are estimated using heteroskedasticity-robust standard errors, clustered by village. We estimate sharpened q-values to for the false discovery rate across dimensions of heterogeneity, within each panel. After this adjustment, no heterogeneous treatment effect is significant at the 10% significance level.

Figure A.9: Distribution of Heterogeneous Treatment Effects Estimated Using Random Causal Forests



Notes: This figure shows histograms of heterogeneous treatment effects estimated using the random causal forest. The outcome in the left-hand column is the aspirations index, defined in Figure 1. The outcome in the right-hand column is the summary economic index, defined in Table 2. The x-axis represents the treatment effects.

Table A.18: Tests for Treatment Effect Heterogeneity Using Random Causal Forests

	Asp&Plan	Cash	Combined
Panel A: Economic index			
p-value for null hypothesis: forest fits data	0.98	0.98	0.98
p-value for null hypothesis: forest detects no HTEs	0.82	0.15	0.81
Panel B: Aspirations index			
p-value for null hypothesis: forest fits data	0.96	0.96	0.85
p-value for null hypothesis: forest detects no HTEs	0.64	0.99	0.64

Notes: This table shows results of tests for heterogeneous treatment effects based on random causal forests. All cells display p-values for testing the listed hypotheses. Each column corresponds to a different treatment arm. Each row corresponds to a different null hypothesis. The first row in each panel reports a goodness of fit test; we fail to reject the null hypothesis that the forest estimated using the training data fits testing data. In the second row, we fail to reject the null hypothesis that the effects over all forest cells are jointly equal. The forest uses the same nine baseline dimensions of heterogeneity shown in Figure A.7. The outcome in the first panel is the summary economic index, defined in the footnote to Table 2. The outcome in the second panel is the aspirations index, defined in the footnote to Figure 1. Inference uses a village-clustered bootstrap following Semenova and Chernozhukov (2021).

0 to 0.18 standard deviations, with almost all estimates falling between 0.05 and 0.15. The workshop and combined effects on the economic index are almost all between -0.05 and 0.2 standard deviations, while the cash effects are slightly higher.

We use these estimates to conduct an omnibus test for the presence of heterogeneous treatment effects, following Tibshirani et al. (2022). We fail to reject the null hypothesis that the treatment effects in all cells estimated by the forest are equal (Table A.18, row 2 of each panel) and fail to reject a goodness-of-fit test for the forest itself (row 1 of each panel).

A.4 Participant Goals

The aspirations and planning workshop encourages participants to set goals for their future. Our aspirations measures provide quantitative proxies for these goals in general domains: assets, income, and education. Here we describe the relationship between our survey measures of aspirations and the actual goals that participants describe during the facilitated exercises. In the workshop and combined groups, one fieldworker observed the group while respondents presented their goals for five years' time and recorded three goals for each respondent. We code the open-ended responses using STATA's regular expression function, regexm, to filter for a given string in the open-ended goals variable. We define a dummy = 1 if a participant has a goal which contains the string. We do not quantify goals in this analysis, as most participants expressed their goals in qualitative terms. We record goals only for participants in the workshop and combined interventions. The placebo exercises did not include any goal-setting discussion that we could have used to record goals.

Few respondents set goals for the same activities shown in the videos, reinforcing the evidence from Section 6 that the treatment effects of the aspirations and planning intervention are not driven by mimicry. Only 3% of respondents set goals in tailoring, Judy's core activity in the video (Table A.19). Only 1% set a savings goal and 7% a goal related to farming infrastructure, although savings and building a greenhouse were described in the videos. Many (48-52%) set goals related to chickens, but this is unlikely to reflect mimicry as 80% of households in the workshop group at baseline have a chicken, as do 78% in the combined group. There is little evidence of differential goal-setting between the workshop and combined groups, with respondents setting very similar goals.

We find positive, statistically significant relationships between a dummy for having a goal in a domain and an individuals' level of aspirations in that domain. The regression coefficients in bivariate regressions of goals in a domain on aspirations in that domain are large and positive (Table A.20), suggesting our measures of aspirations are informative proxies for having a specific goal in that domain, consistent with psychological literature suggesting aspirations and long-term goals are similar conceptually (Locke and Latham, 2002).

Table A.19: Proportion of Respondents Reporting Goals by Domain

	(1)	(2)
Keywords	% Reporting goal workshop group	% Reporting goal combined group
Productive assets	0.96	0.93
Chicken, chick, hen, poultry	0.52	0.48
Goat	0.03	0.03
Sheep	0.01	0.02
Cow, cattle	0.33	0.32
Farming infrastructure[(a)]	0.07	0.07
Income		
Small business goals[(b)]	0.36	0.37
Income-generating farming goals	0.77	0.80
Tailoring goal	0.03	0.04
Savings and credit	0.01	0.01
Savings, save, ROSCA	0.01	0.01
Credit, loan	0	0
Housing[(c)]	0.87	0.85
Education	0.51	0.50
Primary and secondary schooling[(d)]	0.50	0.49
Tertiary education[(e)]	0.06	0.06

Notes: Bolded row titles represent indicators for any subset of groupings of the reference categories beneath them. Proportions do not necessarily sum within reference categories, as often respondents will nominate more than one of each of the members of a category. Proportions do not sum to 100 as participants set more than one goal. [(a)] is made up of the keywords "hoe, irrigation, fence, mill, tractor, greenhouse, tank, plot". [(b)] is made up of "retail, trade, business, tailor, tailoring, shop, sell, fish, rent, boda, motorbike cereal, omena, mala, commercial". [(c)] is made up of "house, home, roof, walls, floor, tin, simba, kitchen, room". [(d)] is made up of "school, educat, educate, education, schooling and any keywords in [(e)]. [(e)] is made up of "university, college, diploma, tertiary".

Table A.20: Relationship between Domain-Specific Aspirations and Goals

	(1)	(2)	(3)
	Aspirations measures		
Dummy if has goal	Education (years)	Income (USD PPP)	Assets (USD PPP)
Education goal	0.25		
P-value	0.00		
Income goal		739	
P-value		0.02	
Asset goal			1376
P-value			0.42

Notes: This table shows the relationship between endline aspirations measures and the presence of at least one elicited goal in the same domain. The goal definitions are specified in Table A.19. We group some of the goals into being related to education, income or assets to correspond to dimensions of the aspirations measure. The reported values are coefficients and p-values from OLS regressions with no covariates and heteroskedasticity-robust standard errors. The regressions use data from the workshop and combined intervention groups.

B Additional Information about Data Collection, Sampling & Eligibility, Treatment Assignment & Receipt

B.1 Sources of Data and Sample Eligibility

We collected five types of data: (1) a **village census** of elders in 415 villages; (2) a **household census** of 41,322 households in these villages; (3) a **baseline survey** of 8,309 sampled households; (4) an **endline survey**, aimed at the same households and (5) **price surveys** throughout the study period. Baseline surveys ran from April 2016 to March 2017. Interventions ran from November 2016 to July 2017. Endline surveys ran from May 2018 to February 2019. Interventions were not run in a sublocation until we had completed baseline surveys for all villages in that sublocation.

Census: We use the village census to collect village-level variables for stratifying the village-level randomisation. We use the household census to determine study eligibility and draw the baseline sample. Of the 41,322 households censused, 32,964 participated in the study, 4,677 refused to participate in research, and 3,681 were not at home.

Identifying poor households and drawing baseline sample: Of those who chose to participate, the group relevant for our study is those who meet GiveDirectly's means test for living in poverty. Households are classified as poor if they met any one of four simple criteria: (1) they had per capita housing space less than 62,000cm^2; (2) they had a mud floor and no mobile telephone;[46] (3) they had a mud floor and the household head was a widow; or (4) the household included an orphan. GiveDirectly has found these criteria to be strong predictors of living below poverty lines defined in terms of consumption expenditure and this is true in our data. Approximately 43% of households in the study villages met the eligibility criteria for living in poverty.

Our study sample excludes some means-tested-poor households: those which do not contain an adult female (4.5%) and those which are polygamous (11%), due to difficulties associated with household definition. These households were still eligible for GiveDirectly transfers

From the remaining households, we sample roughly 20 households per village with replacement: if one of the 20 target households could not be found or refused to participate, the field officers included one household on the reserve list as a replacement. Sampling probabilities vary by village because we don't sample exactly the same share of treatment-eligible households in each village. None of the descriptive or causal results in the paper change substantively when we reweight the data to account for variation in sampling probabilities.

Poverty status: The majority of households in our sample are included because they meet

[46] Households who did not have a mobile phone in the census and were offered a cash transfer were also offered a mobile phone, the cost of which was deducted from the transfer.

GiveDirectly's first two criteria. 66% of households classified as eligible in our sample meet the first means test criterion, small houses; 25% meet the second criterion, reflecting low levels of housing quality and asset ownership. 35% meet criterion 3 (widows with mud floors) and 29% meet criterion 4 (the household contains an orphan). Our survey data shows widows with mud floors and households containing orphans are also very likely to be poor: 85% of households who meet these criteria are below the World Bank poverty line in 2018 USD PPP for Kenya, compared to 90% of households who meet criteria 1 and 2.

Attrition from baseline to endline: We define the study sample as all households that completed the baseline survey. In latter rounds of data collection, households that refused to participate or could not be located are treated as attriters. We attempt extensive tracking of all migrants within and outside the study area, including to nearby cities and towns. We have a total response rate of 87.1% of baseline households. We surveyed 84.7% of baseline respondents at endline. In another 2.4% of households we could not survey the baseline respondent but did survey a proxy household representative. Of the endline sample, 1.2% are surveys with migrants and 0.4% are households that split between baseline and endline. For split households, we surveyed the original respondent and a representative from the other part of the split household. For economic measures, we use proxy responses if the respondent cannot be found and we average both responses for split households. For psychological measures, we only use responses from the baseline respondent. Our main findings are robust to alternative ways of handling proxy responses and split households, partly because these cases are rare.

Table B.1 shows that **attrition does not differ by treatment assignment** (column 1). Attrition is slightly lower for larger households and respondents with higher values of the self-beliefs index (column 3). However, attrition does not differ by treatment $\times$ baseline household characteristics (column 5), showing that the composition of the sample remains similar between groups.

Price surveys: We also conducted baseline and endline price surveys of 55 markets in the study area to collect prices for some commonly purchased goods and services, which we use to construct some economic measures. Baseline price surveys ran from August to November 2016. Endline price surveys ran from May to September 2018.

B.2 Treatment Assignment and Treatment Receipt

Randomisation protocol: We conducted the randomisation using data from the village and household censuses before any baseline surveys began. We stratify randomisation on location (an administrative division in Kenya containing roughly 10-50 villages); village size (a dummy for if village size exceeded the sample median); and a measure of household asset ownership at village level (see Table B.2 for descriptives). Within each block, we randomly assigned villages

Table B.1: Relationship between Attrition, Treatment Assignment, and Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Asp&Plan	0.020	(0.014)			0.055	(0.056)
Cash	0.009	(0.013)			0.027	(0.050)
Combined	-0.001	(0.012)			0.064	(0.049)
Household Size			-0.011***	(0.002)	-0.007*	(0.004)
Age			-0.001*	(0.000)	-0.008	(0.005)
Self-beliefs Index			-0.013***	(0.004)	-0.006	(0.005)
Non-land Assets			0.004	(0.003)	-0.002	(0.005)
Consumption			0.001	(0.001)	-0.000	(0.001)
At Least Primary Education			0.005	(0.008)	-0.000	(0.001)
At Least Secondary Education			0.019	(0.017)	-0.000	(0.001)
Married			0.018*	(0.010)	-0.001	(0.001)
Household Size * Asp&Plan					-0.009	(0.008)
Household Size * Cash					0.000	(0.010)
Household Size * Combined					-0.016	(0.011)
Age * Asp&Plan					0.002	(0.010)
Age * Cash					0.000	(0.006)
Age * Combined					0.009	(0.008)
Self-beliefs Index * Asp&Plan					0.001	(0.008)
Self-beliefs Index * Cash					0.004	(0.008)
Self-beliefs Index * Combined					0.001	(0.003)
Non-land Assets * Asp&Plan					-0.001	(0.004)
Non-land Assets * Cash					0.002	(0.004)
Non-land Assets * Combined					-0.001	(0.004)
Consumption * Asp&Plan					0.003	(0.015)
Consumption * Cash					-0.013	(0.021)
Consumption * Combined					0.012	(0.024)
At Least Primary Education * Asp&Plan					0.004	(0.023)
At Least Primary Education * Cash					0.044	(0.033)
At Least Primary Education * Combined					-0.025	(0.046)
At Least Secondary Education * Asp&Plan					-0.022	(0.048)
At Least Secondary Education * Cash					-0.042	(0.046)
At Least Secondary Education * Combined					0.008	(0.017)
Married * Asp&Plan					0.039	(0.028)
Married * Cash					0.001	(0.026)
Married * Combined					-0.002	(0.024)
P: All arms = 0	0.362				0.869	
P: cash = asp&plan	0.411				0.374	
P: cash = combined	0.433				0.612	
P: asp&plan = combined	0.114				0.662	
Placebo mean	0.122		0.122		0.122	
# obs	8,309		8,309		8,309	

Notes: This table shows the relationship between attrition, treatment assignment, and prespecified baseline covariates. All columns show regressions of a household-level indicator for not being surveyed at endline on treatment arm indicators (cols 1-2); baseline covariates (cols 3-4); and treatment arm indicators, baseline covariates, and their interactions (cols 5-6). All regressions include sublocation fixed effects. The consumption and asset aggregates are measured in constant 2018 USD PPP (000s). The self-beliefs index consists of growth mindset, self-efficacy and internal locus of control scales. If a baseline covariate is missing, we replace the missing values with the sample mean and include a missing data indicator. Heteroskedasticity-robust standard errors, clustered at village level, are reported in parentheses. *; **; and *** denote significance at the 10; 5; and 1 percent levels respectively.

to the four experimental arms. Some block sizes were not multiples of four. We took the residual villages from these blocks, combined them with other villages in the same location and same asset category, and randomised within these units.

Balance tests: Table B.2 reports the results of balance tests in the baselined sample. All tests compare the mean values of baseline village- and household-level covariates between the placebo group and each treatment arm (columns 3, 5, and 7) or jointly across all four arms (column 8). The test results are consistent with random assignment: we reject equality across all four arms at the 10% level for only 1/25 tests and at the 5% level for 0/25 tests.

Treatment receipt: Table B.3 reports the relationship between treatment assignment and treatment receipt. Compliance is the same within the two pairs of arms used for our two main comparisons: between the aspirations and planning workshop and the placebo workshop arms, and between the cash and combined arms, respectively (Table B.3). So any differences between these pairs of interventions do not result from differences in intervention take-up between the two groups.

Columns 5-8 show statistics for only endlined households, on which we focus because this is the sample used to estimate treatment effects. The first row shows the number of households assigned to each treatment group. Panel A shows the share of households in each treatment group (in columns) that received each possible combination of treatments, using data capturing which households are present at a workshop (in our records) or receive cash (using GiveDirectly's records of payments). We do not show separate rows for receiving the aspirations and planning and placebo workshops because households could only receive whichever one of these interventions was assigned to their village. Panel B shows differences in treatment receipt rates between relevant pairs of experimental arms.

Approximately 90% of endlined households in each of the four groups receive the workshop they are assigned. In the placebo group, 89% of endlined households receive the workshop that they are offered ('Placebo' row of Panel A, column 5). The figure is 90% for the Asp&Plan workshop i.e. receipt differs by only 0.6 percentage points with $p=0.66$ ('Workshop Only' row of Panel B, columns 5/6). Any differences in outcomes between these groups do not arise because of differences in attendance at the workshop.

Table B.2: Baseline Summary Statistics and Tests of Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Placebo	Asp&Plan		Cash		Combined		F-test	
	Mean	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	p-value	# obs
Panel A: Village-level Characteristics from Census									
Has Primary School	0.490	-0.010	0.890	0.057	0.427	0.028	0.702	0.802	415
Has Market	0.288	-0.075	0.232	-0.040	0.528	-0.028	0.669	0.685	415
Has Clinic	0.163	-0.075	0.129	-0.048	0.323	0.002	0.964	0.294	415
Number of Households	96.3	-2.73	0.679	-5.35	0.349	0.062	0.993	0.756	415
Mean Household Asset Score	0.030	-0.032	0.515	-0.017	0.728	-0.022	0.649	0.929	415
Floor Material is Mud or Organic†	0.666	0.023	0.158	-0.003	0.872	-0.009	0.569	0.187	415
Roof Material is Grass, Leaves or Other†	0.054	-0.006	0.297	0.000	0.976	0.003	0.631	0.420	415
Walls Material is Unburnt Bricks or Mud†	0.846	0.005	0.672	0.003	0.821	0.001	0.954	0.973	415
Drinking Water is Piped/Well†	0.385	0.047	0.089	-0.012	0.705	0.020	0.526	0.206	415
Lighting is Electricity†	0.284	0.004	0.749	0.008	0.492	-0.003	0.787	0.796	415
Panel B: Eligible Respondent Characteristics from Census									
Married	0.584	0.039	0.027	0.018	0.310	0.001	0.961	0.093	8,309
Age	40.8	-0.603	0.283	-0.866	0.153	-0.808	0.197	0.485	8,302
At Least Primary Education	0.423	0.018	0.253	0.026	0.109	0.024	0.123	0.345	8,274
Household Owns a Mobile Phone	0.741	-0.008	0.628	0.017	0.265	0.011	0.490	0.441	7,743
Panel C: Eligible Household Characteristics from the Baseline									
Household Size	5.31	0.075	0.388	0.031	0.720	0.021	0.818	0.845	8,309
Dependency Ratio	1.35	-0.004	0.923	0.023	0.537	0.026	0.500	0.802	8,308
Number of Household Members Under 16	2.85	0.020	0.751	0.052	0.438	0.032	0.649	0.890	8,309
Revenue Aggregate	1,834	-71.0	0.381	32.0	0.693	38.6	0.674	0.572	8,309
Consumption Aggregate	4,331	-78.1	0.470	-83.2	0.420	-98.7	0.359	0.778	8,295
Investment Aggregate	699	-125	0.067	-48.6	0.507	-40.2	0.661	0.228	8,309
Non-land Asset Aggregate	1,230	-7.57	0.887	-0.049	0.999	-3.80	0.946	0.999	8,309
Total Household Labour Supply (Days)	431	-3.50	0.808	-6.72	0.646	-0.218	0.988	0.965	8,283
Education Expenditure	439	7.00	0.779	-10.2	0.717	-13.0	0.589	0.853	6,958
Index of Self-beliefs	0.000	0.013	0.789	0.051	0.246	-0.031	0.520	0.395	8,270
Index of Aspirations for Future Outcomes	0.000	0.025	0.534	0.012	0.742	0.063	0.104	0.357	8,283

Notes: The table reports balance tests for characteristics measured in the village census, household census, and baseline surveys. Panel A reports regressions at the village level. Panel B reports characteristics of eligible respondents who are the primary women in eligible households. Panel C reports household-level characteristics. All balance tests are implemented by regressing the characteristic on a vector of treatment assignments and sublocation fixed effects. The regressions use one observation per village for the village-level characteristics and one observation per household for the household- and respondent-level characteristics. Inference is performed using heteroskedasticity-robust standard errors, clustered by village for regressions with household- or respondent-level characteristics. Column (1) reports the placebo mean for each characteristic. Columns (2)-(7) report treatment arm-specific coefficients and p-values. Column (8) reports the p-value from test of joint equality of means for all four treatment arms. Column (9) reports the number of observations. The average number of households in each village that completes the census is 75. The household asset score is constructed using principal component analysis on indicators for household ownership of a telephone, bicycle, solar panel, TV, fridge, radio, watch/clock, motorbike, truck and iron box (charcoal or electric). Economic variables in Panel C are measured in constant 2018 USD PPP annually. The dependency ratio is the number of household members under 16 divided by the number of members 16 or above. Outcomes with a † sign denote village-level proportions constructed from household-level data.

Similarly, any differences in outcomes between the cash and combined groups are unlikely to arise because of differences in workshop attendance or cash transfer take-up between these groups. The cash and combined groups have four different treatment receipt measures. They can receive neither the cash transfer nor a workshop ('None' row of Panel A); cash but not a workshop ('Cash Only' row of Panel A); a workshop but not cash ('Workshop Only' row of Panel A) or both interventions ('Cash and Workshop' row of Panel A). Receipt of the workshops is similar between the cash and combined arms: respectively 90 and 91% of endlined households receive the workshop (sum of 'Asp&Plan only', 'Placebo Only' and 'Cash and Workshop' rows, columns 7 and 8). Receipt of the cash transfers is also similar between the cash and combined arms: respectively 79 and 81% of endlined households have a payment record for having receive the cash transfer (sum of rows 'Cash Only' and 'Cash and Workshop', columns 7 and 8). The differences in the portion of the cash and combined arms in each of the 'None', 'Cash Only', 'Workshop Only' and 'Cash and Workshop' categories range from 0.2 to 1.7 percentage points and no difference is significant at conventional levels with $0.54 < p < 0.84$ (Panel B, columns 7/8). A further 4.5–5% of households in both arms may have received cash but we do not have a payment record for them in GiveDirectly's data (Panel C).

Unlike many programme evaluations, we can also document reasons for households not receiving the cash transfer programme, among households for whom we have GiveDirectly records in both arms (Panel C). The portion of the sample who do not receive transfers for different reasons is similar in the cash and combined groups, suggesting similar composition of these groups. Treatment receipt of any treatment is also not clearly related to households' baseline characteristics (analysis not shown). The only clear relationship is that larger households are more likely to receive cash transfers, potentially because they are more likely to have an adult at home and available to meet GiveDirectly's census team.

Perhaps surprisingly, there is higher take-up of the once-off workshop (roughly 90%) than the cash transfer (roughly 80%). This does not affect internal validity: we do not seek to compare the cash and Asp&Plan workshop except in the benefit-cost comparisons, where we compare benefits and costs accounting for all considerations that affect programme operation in a real-world setting.[47] But understanding the nature of take-up might affect how we interpret the ITT estimates in the cash and combined arms (for example, whether the findings from this cash transfer programme generalise to other transfer programmes).

Roughly 10% of endlined households are not found by GiveDirectly – the adult female is not at home, has left the village, is found upon revisit to be ineligible or cannot be found for

[47]This has a limited effect on our comparison of the two interventions' benefit-cost ratios because lower cash take-up lowers both the numerator – because the average benefits are intention-to-treat effects – and the denominator – because the average costs are per person offered each intervention – relative to full take-up.

Table B.3: Treatment Receipt Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline Sample by Treatment Group				Endline Sample by Treatment Group			
	Placebo	Asp&Plan	Cash	Combined	Placebo	Asp&Plan	Cash	Combined
# households	2,012	2,057	2,085	2,157	1,767	1,766	1,814	1,896
Panel A: Intervention Receipt Rates (%)								
None	16.20	16.29	14.92	11.54	10.64	9.97	7.22	5.80
Cash only	0.05	0.00	2.59	2.83	0.06	0.00	2.65	2.80
Workshop only	83.75	83.62	14.15	14.79	89.30	89.92	13.45	13.03
Cash and workshop	0.00	0.10	68.35	70.84	0.00	0.11	76.68	78.38
Panel B: Test for Equal Treatment Receipt Rates between Groups (p-values)								
Cash only		-		0.730		-		0.844
Workshop only		0.946		0.764		0.664		0.832
Cash and workshop		-		0.485		-		0.538
All		-		0.231		-		0.722
Panel C: Cash Transfer Receipt Rates and Reasons for Non-receipt (%)								
Received cash	-	-	70.94	73.67	-	-	79.33	81.17
Ineligible	-	-	3.07	2.83	-	-	2.87	2.85
Not found/home	-	-	2.30	2.32	-	-	1.71	1.58
Left village	-	-	0.96	1.21	-	-	0.66	0.84
Refused	-	-	18.13	15.25	-	-	10.64	9.12
Unknown/other	-	-	4.60	4.73	-	-	4.80	4.43

Notes: This table shows receipt of treatment for households in each treatment arm. Columns (1), (2), (3) and (4) are statistics for the baseline sample while columns (5), (6), (7) and (8) are for the endline sample. Panel A shows the percentage of each treatment group that received each of the four possible treatment combinations: no treatment (row 1), only cash (row 2), only the Asp&Plan/placebo workshop (row 3), or both cash and the Asp&Plan/placebo workshop (row 4). 'Workshop' in rows 3 and 4 refers to completing the aspirations and planning workshop or placebo. Panel B shows p-values from tests across pairs of treatment groups for equal rates of treatment receipt: only cash (row 1), only the Asp&Plan/placebo workshop (row 2), and both cash and the Asp&Plan/placebo workshop (row 3). The fourth row shows the p-value from a joint test of equality of all four treatment receipt rates. Each p-value is centred between the columns corresponding to the two groups on which the test is run. Panel C shows the percentage of the cash-assigned treatment groups that received cash (row 1 of the panel) or did not receive cash for each reason (rows 2-6). Inference is performed using heteroskedasticity-robust standard errors, clustered at village level.

other reasons (rows 2-4 of Panel C). This plausibly reflects difficulties of tracking households in rural settings in all four arms and is similar to the portion of respondents who do not take up the workshops. A further 10% of our endline sample are documented by GiveDirectly as refusing the cash transfers. Reasons included that households did not trust GiveDirectly (75%), were told by someone else not to take the transfer (24%) or did not need money (1%).

Both the rates and reasons for refusal are fairly common for programmes of this nature. There is little published data on *rates* of refusals in unconditional cash transfer programmes in

developing countries: two published studies of GiveDirectly cash transfers do not report receipt rates (Egger et al., 2022; Haushofer and Shapiro, 2016), nor does the prominent study by Baird et al. (2011). But programmes which track take-up suggest this rate of refusal of cash or asset transfers is not unusual. For example, the multi-country study by Banerjee et al. (2015) reports that a bundled antipoverty programme had a refusal rate of 48% in India and close to zero in other countries. In the US, non-participation among eligible households is 66% for the Temporary Assistance for Needy Families and 32% for Supplemental Security Income, two cash transfer programmes (Ribar, 2014). The *reasons* for refusal we document are also similar to those documented by many other programmes and hence are unlikely to mean that conclusions cannot be generalised: a recent Cochrane review finds lack of trust in programme providers and beneficiaries not wishing to be identified as poor are common reasons for refusal (Atkins et al., 2020).

C Additional Information about Workshop Interventions

C.1 Summaries of Video Content

All videos are posted at https://mbrg.bsg.ox.ac.uk/aspirations-and-goal-setting-video-intervention. The aspirations and planning videos tell stories about the lives of Judy and Josefine. Judy and and her partner Oyoo are smallholder farmers with few assets, and are expecting a child. They discuss struggling to get by and what they want for the future. They decide that within five years, they want to put an iron roof on their house, for their children to complete school and for Judy to start a business. They set intermediate goals to save 100 shillings each week, grow more vegetables to sell at market, purchase a plot within a year. They put money in a small container to save. Judy learns to sew, overcoming some obstacles to do so, and starts a successful business making clothes. They succeed in buying an iron roof. She adjusts her business plan to deal with competition from cheap imported clothing and her business prospers. Eventually, they send their child to university.

Josefine is a teacher and farmer. She tells her life story of how she came to be successful. She begins by remembering that she used to beg for money as a child and work as a casual day labourer. Her teacher describes how she dropped out of school several times. Another woman describes teaching Josefine to weave baskets to sell at market. Her husband describes how she saved money from this to go to high school. She explains how she learned conservation farming to improve the productivity of her plot. Her husband describes how she eventually started her chicken-rearing business, despite five failed attempts, including when she overfed her chickens and they did not lay eggs. She outlines plans to build a greenhouse. She also describes her struggles to succeed at teacher training college, when she was much older than others and struggled to learn, but persisted and achieved good grades. She encourages viewers to continue learning throughout their lives.